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Pages

Frontispiece. Portrait of IMMANUEL KANT to celebrate the second centenary of his birth at Königsberg on the 22nd of April 1724 [after the painting of DÖBLER (1791)]. . . . . .

I. *Main Articles* (4).

E. J. HOLMYARD (*Bristol*). — A critical examination of BERTHELOT's work upon Arabic chemistry 479
GIUSEPPE GABRIELI (*Roma*). — ALI IBN RIDWÁN. 500
FLORIAN CAJORI (*Berkeley, Calif.*). — Note on our sign of equality (with one plate) 507
HEINRICH WIELEITNER (*Augsburg*). — Zur Geschichte der gebrochenen Exponenten 509

II. *Notes and Correspondence* (8).

History of Science Society. — Statutes. Officers and Council Members for the First Year (1924). First list of Foundation Members. First annual meeting (F. E. BRASCH, Washington) 521
Queries and Answers edited by FREDERICK BARRY (New York). 533
Teaching of the History of Science and Personalia edited by H. E. BARNES (Northampton, Mass.). 534
VICTOR GOLDSCHMIDT und JULIUS RUSKA (Heidelberg). — Institut für Geschichte der Naturwissenschaft in Heidelberg 538
National Museum of Engineering (Washington 1924) 539
Oxford Museum of the History of Science (1924) 542
Harvard Museum of the History of Science 543
G. SARTON's Introduction to the History of Science and Civilization . . 543

III. *Reviews* (17).

		Page
S. XV.	Joannes de Ketham. Fasciculus Medicinæ 1491. Facsimile edited by Karl Sudhoff and C. Singer. Milan, 1924 (G. S.)	547
S. XIX.	Fournier d'Albe, E. E. The life of Sir William Crookes. London, 1923 (G. S.)	549
Egypt.	Peet, T. Eric. The Rhind mathematical papyrus. London, 1923 (G. S.)	553
Greece.	Robin, Léon. La pensée grecque et les origines de l'esprit scientifique. Paris, 1924 (L. G.)	557
Islam.	Becker, C. H. Islamstudien. Vom Werden und Wesen der islamischen Welt. Leipzig, 1924 (J. Ruska)	559
	Gaudefroy-Demombynes, M. La Syrie à l'époque des Mamelouks d'après les auteurs arabes. Paris, 1923 (D. B. Macdonald)	561
	André, P. J. L'Islam et les races. 2 vol. Paris, 1922 (D. B. Macdonald)	563
Biology.	Rignano, Eugenio. La mémoire biologique. Paris, 1923 (L. G.)	564
Geology.	Mager, Henri. Une science nouvelle : la science des vibrations atomiques. Paris, 1923 (L. G.)	565
Mechanics.	Lecat, Maurice. Bibliographie de la relativité. Bruxelles, 1924 (G. S.)	567
Medicine.	Garcia del Real, E. Historia de la medicina en España. Madrid, 1921 (L. G.)	568
Philosophy	Bergson, Henri. Durée et simultanéité. 2e éd. Paris, 1923 (H. M. Sheffer)	570
Psychology.	Claparède, Edouard. Comment diagnostiquer les aptitudes chez les écoliers. Paris, 1924 (L. G.)	571
Science, philosophy.	Dingler, Hugo. Die Grundlagen der Physik. Synthetische Prinzipien der mathematischen Naturphilosophie. Berlin, 1923 (H. M. Sheffer)	572
	Elsbach, A. C. Untersuchungen über das Verhältnis der modernen Erkenntnistheorie zur Relativitätstheorie. Berlin, 1924 (H. M. Sheffer)	573
Sociology.	Ferrière, Ad. La loi du progrès en biologie et en sociologie. Paris, 1915 (L. G.)	574
Superstition.	Sears, C. E. Days of delusion. Boston, 1924 (G. S.)	576
Zoology.	Wheeler, W. M. Social life among the insects. New York, 1923 (R. M. May)	578
	Forel, Aug. Le monde social des fourmis. 5 vol. Genève, 1921-23 (R. M. May)	578

IMMANUEL KANT

From the « Corpus Imaginum » of the Photographische Gesellschaft, Charlottenburg, after the painting of Dobler, 1791.

Zwei Dinge erfüllen das Gemüth mit immer neuer und zunehmender Bewunderung und Ehrfurcht, je öfter und anhaltender sich das Nachdenken damit beschäftigt: der bestirnte Himmel über mich und das moralische Gesetz in mir (*Kritik der praktischen Vernunft*, Riga, 1788).

A critical examination of Berthelot's work upon Arabic chemistry [1]

The notable interest in the work of mediæval Muhammadan chemists which has arisen in the last few years probably has its origin in a fuller appreciation of the fact that the numerous puzzling problems presented by European chemistry of the early middle ages cannot be solved until a thorough investigation has been made of the rise and development of chemistry in Islam, whence it was imported to Christendom during a long period beginning in the first half of the twelfth century.

Two of the earliest historians of chemistry to realise the importance of this study were BERTHELOT and Prof. EILHARD WIEDEMANN. Whilst the scholarly researches of the latter have been comparatively neglected in this country until recently — probably because they were generally published in somewhat obscure local German periodicals — BERTHELOT's monumental work in three volumes (*La Chimie au moyen âge*, Paris, 1893) at once attracted attention, and has been used — for the most part quite uncritically — by all later writers. It seems, therefore, of fundamental importance that we should arrive at a clear knowledge of the length to which BERTHELOT may be safely followed, of the accuracy of his conclusions and of the relation which the ground he covered bears to the whole field.

There can be no doubt, to my mind, that BERTHELOT's work is deserving of the most generous praise. All those who since his time have studied Islamic chemistry owe him a great debt, acknowledged or unacknowledged, and VON LIPPMANN's section on the subject, in his invaluable *Entstehung und Ausbreitung der Alchemie*, is based

(1) Reprinted with the editor's and author's kind permission from *Chemistry and Industry Review*, published by the Society of Chemical Industry, vol. 42, 958-963, 976-80, London, 1923.

principally upon *La Chimie au moyen âge*. That remarkable genius, MORITZ STEINSCHNEIDER, expresses his opinion of BERTHELOT's researches in the following words (1) : « Mit dem grossartigen Werke van BERTHELOT, *La Chimie au moyen âge* (1893), dessen 3 Band die arabische Chemie, bez. Alchemie, behandelt und einige der ältesten arabischen Schriften mit französischer Uebersetzung von O. HOUDAS enthält, mögen Physiker, die sich für Geschichte ihrer Wissenschaft interessieren, den Gegenstand für erschöpft halten; ja auch diejenigen, welche die Kulturgeschichte der Araben und deren Bedeutung für die Geschichte des Mittelalters nicht unterschätzen, dürften die Resultate BERTHELOT's als für ihren Zweck ausreichend ansehen und ohne weitere Forschungen verwerten. »

Coming from such an eminent authority, this praise is high indeed, and although — however diffidently — we may protest against an Arabist expressing such a dogmatic opinion on what is primarily a matter for chemists to decide, it comes as somewhat of a shock to find that Prof. VON LIPPMANN (2) passes extraordinarily severe strictures upon « BERTHELOT als Historiker ». VON LIPPMANN, of course, wrote his book during the late war, when feeling ran high, and would possibly have employed less scathing terms under normal circumstances; but the fact remains that BERTHELOT's reliability has been seriously impugned, and until the matter is satisfactorily cleared up no historian of chemistry can feel safe in accepting, or rejecting, BERTHELOT's conclusions. It is in the hope of throwing a little light upon this obscure situation that the present article has been written. It will be convenient to arrange the discussion under three heads : — (1) BERTHELOT's qualifications for his task; (2) his choice of material; (3) his treatment of the material chosen, and his conclusions.

(1) BERTHELOT'S QUALIFICATIONS FOR HIS TASK

It will, I think, be generally agreed that the historian of chemistry must primarily be a chemist, for otherwise he is fatally handicapped from the start. No one can dispute BERTHELOT's possession of this essential qualification. Secondly, it does not appear unreasonable to insist that a man who sets out to make *original* investigation of a

(1) Z. D. M. G., 58, 299 (1904).
(2) *Op. cit.*, pp. 647-659.

hitherto unexplored region in the history of a science should have an adequate knowledge of the language and literature of the men whose works he proposes to study. BERTHELOT, however, as is well known, had not even a bowing acquaintance with Arabic, and was therefore forced to rely upon translations. These translations were made for him by Prof. O. HOUDAS, an excellent Arabic scholar famed especially for his translation of BUKHARI's « Traditions of the Prophet ». It would seem, at first sight, as though this solution of the difficulty was comparatively satisfactory, but further consideration will show us (*a*) that, even for the actual work of translation, some knowledge of chemistry is essential, and this HOUDAS did not possess, and (*b*) that to translate a technical work from any language presupposes in the translator an adequate understanding of the technical terms and phrases employed, and an acquaintance with cognate works in the same and other languages. This second point is of especial importance in a subject so obscure as alchemy and in a language of so rich a vocabulary as Arabic. I believe I am correct in saying that Prof. HOUDAS has no special knowledge of Arabic chemical treatises, and that he undertook the work of translation only at the special request of BERTHELOT, backed by the ***Ministère de l'Instruction publique***. My opinion, which I have less hesitation in setting down here in that it is supported by two eminent Arabists, namely, Prof. E. G. BROWNE and Prof. E. BLOCHET, is that properly to understand an Arabic work on chemistry requires both intensive and extensive concentration, and that even the best Arabic scholar would not produce a satisfactory translation of such a work unless he was familiar with many others of the same *genre*. I may, perhaps, quote Prof. BROWNE's own words, taken from his admirable book on *Arabian Medicine* (3) : « I have been greatly hampered in my reading of Arabic medical books by the difficulty of determining the exact scientific signification of many words used in the ordinary literary language in a looser and less precise sense than that which they evidently bear in the technical works in question. Nor is much help to be derived from the mediæval translations of the « Latino-Barbari », who too often simply preserve in a distorted form the Arabic term which they pretend to translate ». If one of the greatest living Arabists, himself a qualified physician, feels this difficulty in dealing with

(3) Camb. Univ. Press. 1921, p. 3.

medical works, at what sort of disadvantage must an Arabist of purely literary training find himself when confronted with texts dealing with alchemy, a subject far more involved than medicine? Prof. BLOCHET remarks (4), « le sujet est extrêmement obscur et la traduction de M. HOUDAS souvent insuffisante ».

In spite of the difficulties with which he had thus to contend, Prof. HOUDAS in point of fact made translations which under the circumstances must be regarded as extremely good, and they certainly are worthy of his scholarship. At the same time they often fail, as might be expected, in those very places in which accurary is essential, namely, the technical words and phrases. To take a case in point : one of JÂBIR IBN HAYYÂN'S books is entitled *Kitâb Abi Qalamûn*. HOUDAS translates (5) this as « un livre à Qalamoc », by reading *ilâ* instead of *abi*, which is in itself an immaterial mistake; but he obviously was not aware of the fact that *Abi Qalamûn* was commonly employed by chemists as a name for the jasper or, perhaps opal, the correct translation of the title thus being « The Book on the Jasper ». Similar mistakes are numerous; thus he treats the word *ustuqus* as a proper noun (6) and leaves it untranslated, whereas anyone acquainted with JÂBIR'S writings knows that *ustuqus* signifies « the four elements » of Aristotelian philosophy; and in the same place he misreads *âs* (myrtle) for *'uss* (base or foundation), thus rendering his translation not merely unintelligible but ridiculous.

HOUDAS' translation thus inevitably suffers from his lack of technical knowledge, so that, even if this were all, we should be compelled to admit that BERTHELOT was beginning his work upon a doubtful basis. But when we read (7) BERTHELOT'S statement, « *J'ai revisé avec soin cette traduction littérale* (8), de façon à tâcher de lui donner un sens intelligible, autant que le comporte ce genre de littérature », and remember that he knew no word of Arabic, we cannot deny that he is wilfully breaking one of the first canons of literary criticism and laying himself open to grave censure. It is, indeed, difficult to see what justification a man could possibly have for believing himself capable of revising a professedly literal translation

(4) Private communication to the author.
(5) *La Chimie au moyen âge*, III, 33.
(6) *Op. cit.*, pp. 32. 33 (« Estaqès »).
(7) *Op. cit.*, p. 6.
(8) Italics mine, — E. J. H.

from a language of which he was admittedly completely ignorant. Especially is this true in the case of a work which was the first of its kind to be translated into a modern European language, and of which the very atmosphere was strange.

There is, however, no need for us to make the uncharitable assumption of Prof. von Lippmann (9), and ascribe this lapse from sound scholarship to Berthelot's egoism and conceit. It is much more likely that he was inspired by an earnest desire to throw as much light as possible upon the subject, and succumbed to the fatal temptation of trying to *make* sense instead of adopting the more toilsome but only sure path of finding out *the* sense by a comparative study of the whole field, or of at least a much wider portion of it.

We have seen, therefore, that Berthelot's whole work on Arabic chemistry rests upon an insecure foundation, since — to use the words of Prof. Ruska — the translations of the Arabic texts upon which he relied suffer from « zahllosen Ungenauigkeiten und durch vorgefasste Meinungen bedingten, irrtümlichen Auslegungen ». There are, however, some factors which tell in the opposite direction, and these must now be considered. It has long been known (*a*) that chemistry was introduced into Islam chiefly from the Greek school at Alexandria, and (*b*) that mediæval European chemistry was a direct legacy of Islam. If, therefore, a man were thoroughly cognisant of both the source and the fruit of Muhammadan chemistry he would have a very valuable qualification for dealing with the latter itself. So far as I am aware, no one has disputed Berthelot's comprehensive knowledge of European chemistry of the middle ages, and we need have no hesitation in saying that the disadvantage under which he lay from having to rely on translations was thus partially neutralised. Partially only, however; for on the one hand he could not be in a position to throw any light on those problems which had their origin in Islam and were often transported bodily and unintelligently into the Latin world, and on the other lay the pitfall of reading into the Arabic works meanings and conceptions which were, in fact, of spontaneous and later European birth. Berthelot

(9) *Op. cit.*, p. 648. « Dieser grosse Mann besass eben auch grosse Schwächen und zu ihnen zählte vor allem eine übermässige Eitelkeit; sein Leitmotiv auf allen Gebieten, die er bearbeitete, war daher der Satz, den der einstige Schüler im zweiten Teile des « Faust » mit den Worten ausspricht : « Die Welt, sie war nicht, eh' ich sie erschuf ».

has, indeed, not been successful in avoiding either of these dangers; on the contrary, he has often failed, in addition, to realise that numerous facts and theories which he believed to be definitely European are to be found in the writings of Muhammadan chemists. In not a few cases the errors which he thus almost inevitably made are of capital importance.

In the other direction we have to gauge the extent of his knowledge of Greek alchemy. In 1888 he published his *Collection des Anciens Alchimistes Grecs*, in which for the first time texts of the writings of these authors were made available. Of the worth of this collection I am not in a position to judge, but it is to be noted that, as in the case of the Arabic works, the translation was not made by BERTHELOT himself, but by Prof. RUELLE, and that the latter's version was subjected to the same treatment by BERTHELOT as was undergone by that of Prof. HOUDAS. One therefore has the uneasy feeling that, here also, one may be on uncertain ground, and although Prof. CASTER has expressed a high opinion of the work, other scholars have not been slow to pass very unfavourable judgments upon it. I think we may, however, be fairly safe in assuming that BERTHELOT had a pretty good general idea of Greek alchemy, and may therefore admit that he was in a position to recognise Greek ideas and influence in Islamic chemistry, though perhaps in matters of detail he was liable to go astray.

To summarise the points made in this section before proceeding to a discussion of BERTHELOT's choice of material, it may be said :

(*a*) BERTHELOT possessed the first essential of a historian of chemistry in that he was a chemist.

(*b*) He had, however, no knowledge of Arabic and was compelled to fall back upon translations.

(*c*) These translations are untrustworthy and inaccurate from a technical point of view—which, in this case, is the vital one.

(*d*) They are further vitiated by the fact that BERTHELOT subjected them to a « revision » which, from the very nature of things, was bound to be uncritical, in order « to make sense »

(*e*) He had an adequate knowledge of mediæval European chemistry, which would help him to understand much of the Arabic work but which would leave him helpless in those very points where help was most needed.

(*f*) He often ascribed to Europe knowledge which came from Islam.

(*g*) He had a better knowledge of Greek alchemy than anyone else, and was therefore able to appreciate Greek influence on Muhammadan chemical thought. But his ideas and scholarship as expressed in the *Collection des Anciens Alchimistes Grecs* have since been severely criticised by those qualified to judge.

(*h*) Finally, one may add that BERTHELOT possessed an inveterate habit of sweeping away as « incomprehensible » anything which he could not himself understand.

Perhaps I may be allowed to give my own experience, for what it is worth. Starting with an implicit faith in BERTHELOT — naïve perhaps, but natural at the sight of his imposing volumes, which give one the impression of finality — I have been gradually led to a profound and progressive distrust, until at the present moment I feel that it is quite unsafe to accept any of his conclusions without first referring to the original sources or without strong collateral evidence. This, of course, does not imply that all are wrong; many are accurate, many others require only slight modification, and *La Chimie au moyen âge* contains a vast amount of material of the greatest value; but the whole is marred by the circumstance that every fact has to be checked, every statement verified, every identification investigated and every translated phrase compared with its original. It is only natural that historians of chemistry, who have for the most part accepted BERTHELOT's work as authoritative, should have drawn thence both the true and the false, and that thus there have crept into the literature errors which will probably prove extremely difficult to eradicate. Even the late Prof. CAMPBELL BROWN, whose excellent *History of Chemistry* has acquired a well-deserved reputation for insight and accuracy, relied entirely upon BERTHELOT for his account of oriental chemistry, with the result that his pages are disfigured by gross inaccuracies, ludicrous transliterations and seriously incorrect conclusions.

(II) BERTHELOT'S CHOICE OF MATERIAL

About 300-400 different Arabic treatises on chemistry are known, and it is probable that a great many more exist in private libraries in Muhammadan countries. The *Bibliothèque Nationale* contains some 30 distinct MSS., and there are several of importance in the library at Leyden. The richest European libraries in this connexion are, however, the British Museum and the National Library at Berlin. The Royal Library at Cairo contains a large number of alchemical

MSS., and I have been told that the King of the Hijaz possesses a fine collection, although I cannot vouch for the accuracy of this statement.

The works vary considerably in both age and importance. The majority range from the eighth century to the fourteenth, though the MSS. themselves are in general much more modern. It is rare to find a MS. older than the fourteenth or fifteenth century, but this is a common feature in Islamic literature and there is good reason to suppose that, in the main, the writings have been accurately transmitted. This does not imply, of course, that instances of pseudepigraphy are rare; what is meant is that, once a work was written, it was usually carefully copied, so that, although — for example — the earliest extant MS. of an eighth century book may be of the thirteenth century, we may with some confidence assume that it has not undergone serious change in the process of transmission (10).

The problem which BERTHELOT wished to solve was two-fold : in the first place he desired to find the relation which existed between the alchemy of the Greeks and that of the early Muslims, and secondly he was anxious to discover whether certain of the Latin works of the early Middle Ages were really, as they professed to be, translations from the Arabic. He also incidentally wished to find whether the Arabic works of JĀBIR IBN HAYYĀN showed any resemblance to those of the Latin GEBER. It is clear therefore, that he intended to limit himself to a study of the earliest and latest periods in the history of chemistry in Islam, and this is in fact exactly what he did, neglecting entirely the intervening period of three or four centuries. This limitation must be borne in mind when we come to criticise his choice of material, and it is necessary to point out that those historians of chemistry who have assumed that BERTHELOT investigated the whole period and gave merely the most important and interesting works in his book, have made a grave error for which the only excuse is that BERTHELOT himself is liable to give one that impression.

BERTHELOT's immediate objective, then, was *(a)* to have recourse « aux ouvrages arabes eux-mêmes et de les comparer avec les textes latins, réputés traduits d'après des œuvres congénères » (11); and

(10) Needless to say, the necessity of a critical study of the internal evidence etc., is not by any means precluded.

(11) *Chimie au moyen âge*, III 5.

(*b*) to have translated from the Arabic those treatises or compilations « où il est particulièrement question des alchimistes grecs, et, d'autre part, les ouvrages qui portent le nom de [JĀBIR IBN ḤAYYĀN] ».

With regard to (*a*), we find that BERTHELOT did not succeed in discovering, or at least did not use, any work that is known in both Arabic original and Latin translation. It is true that up to the present very few of such works have been found, but one at least he had at his disposal in the *Liber Secretorum Bubacaris* (Paris MS. 6514, pp. 101-112), of which the Arabic text occurs in two MSS., one at Leipzig and a second at Göttingen. He gives a summary of the Latin version on pp. 306-310 of volume I of *La Chimie au moyen âge*, and recognises that it is undoubtedly an Arabic work. It is, therefore, strange to find that he omits to give the Arabic text, as he had here ready to hand an excellent piece of material for aiding him to arrive at a definite conclusion on one of the questions under consideration. If the reason for this omission is that he was unaware of the existence of the Arabic MSS., he cannot be acquitted of the charge of negligence, for the Leipzig MS. is very fully described in the catalogue of the City Library published in 1838. If, on the other hand, he knew of the Arabic version (which seems improbable), but considered it unimportant, he showed a serious lack of judgment, for this work of « BUBACHAR » (*i. e.*, ABU BAKR MUḤAMMAD IBN ZAKARIYYA AL-RAZI, or RHAZES) is, as Prof. RUSKA has pointed out, of the greatest value as a compendium of the practical chemical knowledge of Islam in the tenth century.

Of other Latin works which BERTHELOT considered to be, in all probability, translations from the Arabic, he was unable to find originals, and therefore was not in a position to arrive at trustworthy conclusions concerning them. We may therefore, I think, fairly say that as far as regards point (*a*), BERTHELOT left the matter in practically the same state as that in which he found it, and that the whole problem is still to be solved. It is for this reason that the appearance of Prof. RUSKA's promised edition of the Leipzig and Göttingen MSS. of the above work is eagerly awaited.

To turn now to point (*b*), BERTHELOT gives the texts of the following works which bear unmistakable signs of Greek influence : « un livre attribué à OSTANES; un extrait du ms. 1074 (Bibl. Nat.) renfermant une série de citations des alchimistes grecs; le livre de CRATÈS, et le livre de EL-HABIB ». Of these, the last two are taken from the

Leyden MS. 440, which Prof. HOUDAS was inclined to think « n'a pu être écrit postérieurement au VII^e^ siècle de l'hégire et date même plutôt du VI^e^ siècle », whilst the first is the Bibl. Nat. MS. *Arabe* 2605, written partly in the year 1337 A.D. (738 of the Muhammadan era).

Of these it is sufficient here to say that the choice, though small, is quite sufficient for the purpose and on the whole satisfactory; the inclusion of the works by « OSTANES » is perhaps unfortunate, as the OSTANES question is almost as puzzling as that of Hermes himself, and at once introduces many side issues of a mystical and theosophical nature, thus further complicating a problem which is already quite difficult enough. Still, the works which BERTHELOT chose enabled him to prove conclusively what had before been a matter of conjecture (although there was not in reality much doubt about it), namely, that Arabic chemistry is very largely derived from neo-hellenic sources — as is, indeed, definitely stated by the Muslims themselves. BERTHELOT's work here, however, is of value not merely in that it proved this transmission but because it showed also the nature and extent of the ideas that were transmitted, the names that were honoured and some of the theories that were generally accepted. If the treatises of OSTANES and the others are to be taken as typical of the Greek alchemy which was the seed of chemistry in Islam, we can gain a good idea of the progress that was made by the Muslims, for as BERTHELOT remarks, « il y a peu de chose sans doute à tirer de [the books ascribed to OSTANES] pour la chimie positive » (12). The only criticism which need be made of BERTHELOT's choice in this connexion is that as KHĀLID IBN YĀZID is unanimously described as the first Muslim who had translations of Greek alchemical works made, and studied the subject himself with ardour, it would seem that an investigation of his writings might have afforded more definite information than do the works of the anonymous or mythical authors such as CRATES and OSTANES. Much of KHĀLID's chemical verse is easily available, and I believe that his complete (?) *Diwān* is preserved at Constantinople. Unfortunately, KHĀLID has been one of the chief victims of the falsifiers.

It is, perhaps, in his treatment of JĀBIR IBN HAYYĀN that BERTHELOT most signally failed. A proper appreciation of the work of a man

(12) *Ibid.*, p. 15.

who wrote some 500 books cannot be gained by a study of only nine, and these not the most important. I do not propose in this place to offer any comment on the vexed question of the possible identify of JABIR with GEBER, but will confine myself to pointing out that some 80 works attributed to JABIR are still extant, whole or fragmentary, and that a study of them enables one to see clearly the reasons which led later chemists to regard their author with an extraordinary reverence. The choice of JĀBIR's works made by BERTHELOT is entirely misleading and inadequate. The extent of his knowledge was amazing, and to regard him as a bombastic obscurantist, as BERTHELOT does, is to do him a grave injustice. I hope to show later the remarkable scientific knowledge he possessed, but meanwhile I may perhaps refer to a bibliography I have made of his extant works (13), where those who are interested may gain some idea of the negligible relation which exists between the works published by BERTHELOT and those which were available for his study if he had cared to make a search.

To sum up :

1° BERTHELOT made no attempt to study the whole field of Arabic chemistry—an important fact which has, as a rule, not been observed by historians.

2° He neglected entirely the development of chemistry in Islam in its most flourishing period.

3° He confined his attention to three points, (*a*) Arabic originals of Latin works, (*b*) Greek influence, and (*c*) the works of JĀBIR and their relation to the Latin works of GEBER.

4° As regards (*a*) he made scarcely any addition to our knowledge. As regards (*b*), his choice of material was sufficient and he proved his point, although he did not exhaust the subject and his material might have been better chosen. As regards (*c*), he proved that the Latin works could not have been translated *from the Arabic works he published*, but he did not (α) prove that the Latin works were not based on Arabic originals, nor (β) did he give an adequate account of JĀBIR's knowledge, choosing only nine works out of some 80 available.

I shall now deal with BERTHELOT's treatment of his material and with his conclusions, but meanwhile it has been made clear : 1° that, as far as Arabic chemistry is concerned, he is not a safe guide; 2° that he touched only the fringes of the subject; 3° that his choice of

(13) *Proc. Roy. Soc. Medicine*, October, 1923.

material was extremely inadequate, and 4° that in the case of the one chemist of whom he made a professedly detailed study he gave a very inaccurate picture based upon a negligible fraction of the data which are available. This is a serious indictment, but I believe it to be made with justice and to be based, as I have tried to show, upon irrefragable evidence.

(III) Berthelot's Treatment of his Material

The manuscript material which Berthelot employed comprises 13 different works, of which nine are attributed to Jābir ibn Ḥayyān. The whole 13 occupy 205 pages of Arabic type and 180 in the French translation. In addition to the manuscripts, he had at his disposal (*a*) the catalogues of the chief European libraries, many of which contain valuable critical and bibliographical notes, (*b*) the *Kitāb al-Fihrist* in Fluegel's edition, (*c*) the Biographical Dictionary of Ibn Khallikān in English translation, (*d*) the *Prolegomena of* Ibn Khaldûn in French translation, (*e*) the *History of the Philosophers*, by Al-Qifti, (*f*) the *Classes of Physicians*, by Ibn Abi Usaybi'a, and (*g*) the celebrated bibliography, the *Kasfu'l-Zunun*, of Hajji Khalifa; to name only those important sources to which one would naturally turn. Of the last six he claims to have used all except *e* and *f*, but it is clear from his book that whilst he has quoted the whole of the appropriate section of the *Fihrist* and much of the passage in Ibn Khaldûn, he has not drawn very much from Hajji Khalifa, although a Latin translation of the *Kashfu'l-Zunun* was published by Fluegel many years previously (1838-1858). Of the catalogues of libraries he makes scarcely any mention and seems indeed to have neglected this valuable source of information.

The section of the *Kitāb al-Fihrist* (987 A.D.) dealing with alchemy was translated by Houdas and is given on pp. 26-40 of volume I of *La Chimie au moyen âge*. The *Fihrist* is of great value for the history of alchemy, as it gives a list of the early alchemists and the titles of many of their works, but Houdas' version is unprovided with notes or critical apparatus of any kind and contains several inaccuracies in translation and some very doubtful identifications. Thus *Afrāgasūs* is identified (14) with an uncertain *Africanus*, in spite of the fact that the latter name is in Arabic *al-Ifriqiyyu*, whilst

(14) *La Chimie au Moyen Age*, III, 28.

the suggested identification (15) of *Arkaghanis*, as well, with *Africanus* has nothing whatever to recommend it.

In short, the data provided by the *Fihrist* have simply been thrust into the book without any attempt at digestion, and anyone who is carrying out research on the subject will get no assistance from HOUDAS' rendering. This, however, is perhaps beside the point, which is whether BERTHELOT himself found it useful. In so far as the passage had never been translated before, it was no doubt interesting and gave a bird's-eye view of the range of Arabic alchemy of the first three centuries of the Hijra, but its true value (which, as has been remarked above, is considerable) cannot be appreciated, nor can the passage itself be properly used, without a knowledge of a great many of the early alchemical works themselves. It is clear that BERTHELOT realised this, for he leaves the section entirely without comment.

The other authorities are treated in the same cursory fashion, no attempt being made to arrive at a general idea of the course of Arabic alchemy, even up to the time of the Crusades, which was the limit which BERTHELOT set himself. Twelve pages of heterogeneous facts are, indeed, all that he appears to have extracted from the rich bibliographical sources he had immediately available. Even these facts are often incorrectly given. Thus there is no excuse for assigning IBN SINA (AVICENNA) to the twelfth century (16), when the dates of his birth and death are well known to have been 980 and 1037 respectively; the date of the death of AL-TUGHRA'I, definitely given as 1122 (17), is in reality uncertain; IBN AMYAL (or better UMAIL) AL-TEMIMI lived not in the twelfth century A.D. (18) but about 900 A.D.; ABU CASBA IBN TEMMAM AL-IRAQI (19), should be ABU'L-ASBA; DJELDEIK (rather JILDAKI), placed between IBN HASAN ALI (*i.e.*, BURHAN AL-DIN ABU'L-HASAN ALI IBN MUSA IBN ARFA' RA'S), who died probably in 1197, and AL-GHAZZALI (died 1111), lived in the fourteenth century and died at Cairo somewhere about 1360. The famous controversy as to the reality of alchemy arose not in the

(15) *Ibid.*, p. 35.
(16) *Ibid.*, p. 4.
(17) *Loc. cit.*
(18) *Loc. cit.*
(19) *Loc. cit.*

twelfth century (20), but in the first half of the ninth, in which period YA'QUB-AL-KINDI lived. KHĀLID IBN YAZID died in 704, not 708 (21), whilst the date of the death of MASLAMA AL-MAJRITI (22) is not definitely known, and there is good reason to suppose that he never wrote on alchemy at all. Instead of its being the fact that « la plupart de ces traités (of Arabic alchemy) ont disparu » (23), at least two-thirds of those mentioned by HAJJI KHALIFA in his article on *Alkimia* are still extant, although of course a great number of the very early ones have been lost. Of the list of 22 works quoted by BERTHELOT on pp. 41 and 42, about 10 are known is MS.

In dealing with the Latin works on alchemy ascribed to AVICENNA, he has made a great many identifications which are, to say the least, very improbable; thus KING GALUD of Babylon is definitely identified with KHALID (24) without any reason given; ZUCRAT, obviously SUQRAT, or SOCRATES, is suprisingly called ZOROASTER; CLARŌ is given as « CATON »; ZABAIBAR appears to be identified with VERGIL, for reason not given (25) whilst ALPHARABI, who is clearly ABU NASR AL-FARABI, is not identified. The « JAHIE ABENDINON » of AVICENNA's list is unknown, but it has occurred to me that he may have been a son (YAHYA) of the Egyptian alchemist and Sufi DHU'N-NUN, and that « JAHIE ABENDINON » may be a crude translation of YAHYA IBN DHU'N-NUN. I do not know, however, whether DHU'N-NUN ever had a son, and I can find no trace of such a person as an alchemist. DHU'N-NUN himself died in 800 A. D., so that his son would have lived in the ninth century, a date not incompatible with his inclusion in a list of AVICENNA's.

It will be seen that BERTHELOT's introductory matter is meagre, that it has not been properly dealt with, and that it contains numerous inaccuracies which might easily have been avoided, and many doubtful identifications. It is, therefore, of very little help to a study of the manuscript texts themselves, and is often misleading. In other words, the published texts stand practically by themselves and have to be considered on their own merits, except for the light which

(20) *Loc. cit.*
(21) *Op. cit.*, p 2.
(22) *Op. cit.*, p. 3.
(23) *Op. cit.*, p. 5.
(24) *Op. cit.*, p, 301.
(25) It is improbable that AVICENNA knew of VERGIL.

Greek alchemy throws upon them. The manuscripts employed, especially the Leyden MS. 440, were not in a very good state, that volume, indeed, having been « mouillé dans sa partie inférieure », so that « les trois ou quatre dernières lignes sont souvent à moitié effacées, et parfois certains mots ont été complètement rongés par l'humidité ». This rendered decipherment in many places uncertain, and in many others impossible. The major portions of the texts were, however, in good condition and the readings « presque toujours » certain.

The MSS. were transcribed by HOUDAS and printed in Volume III of *La Chimie au moyen âge*, accompanied by a French translation as revised by BERTHELOT according to the peculiar canon above mentioned, namely, with the object of « making sence ». Pages 9-23 give short descriptions of the treatises, together with a few notes. The annotations which accompany the translations deal almost entirely with resemblances of certain passages to passages of Greek alchemists, and the references are usually to the *Collection des Anciens Alchimistes Grecs*. In other words, BERTHELOT did not attempt the task of a proper elucidation of the translations, but confined himself to one definite point. This, as has already been said, he sufficiently proved; nevertheless, on the main question of the nature and development of Arabic chemistry he is perforce silent, and the historian will not derive much help in this direction from a perusal of the book. The works of CRATES, AL-HABIB and OSTANES, given by BERTHELOT, are indeed remarkable, not as being typical of Arabic chemistry, but because they are exceptional. I know of only one other MS. of « OSTANES' » treatise, and I have never come across a reference to those of CRATES or AL-HABIB in JĀBIR, AL-RAZI, BURHAN AD-DIN IBN ARFA' RA'S, ABU'L-QUASIM AL-IRAQI, IBN SINA the pseudo-MASLAMA AL-MAJRITI, AIDAMIR AL-JILDAKI, or in fact, in any other Arabic chemical writer, so that I do not believe that they can have had any considerable influence. Many of the ideas and much of the phraseology in them are of course common, but the works themselves appear to have been almost completely neglected by Muhammadan chemists. On the other hand, some of the books of JĀBIR which are given by BERTHELOT are frequently quoted by later authors, and there is no reason to doubt their authenticity; they do not, however, give an adequate picture of their author.

In using the texts one finds at once the difficulty of referring to any particular passage, as the pages of the MSS. are not numbered,

neither are any indications given in the Arabic of the corresponding pages in the translation. This would not be a drawback in the case of a work divided into numbered sections and subsections, but it is very aggravating in the long and rambling treatises chosen and causes a great waste of time. Moreover, the texts themselves cannot be said to be properly established, since they were made from single manuscripts, often imperfect. With the books of CRATES and AL-HABIB this was inevitable, since no other MSS. are known, but another MS. of the OSTANES is in the British Museum, and MSS. of the « Book of Royalty » and « Little Book of Mercy » are fairly common. The « Books of Mercury » and the other books of JĀBIR are freely quoted by AL-JILDAKI and others, and their texts could thus have been partially checked.

In the first volume of *La Chimie au moyen âge*, BERTHELOT brings to bear his knowledge of mediæval Latin treatises supposed to have been translated from, or based on, Arabic originals, but, as has already been pointed out, he is here attempting to explain *ignotum per ignotius*. In the Latin works it is impossible to separate the indigenous from the foreign, whether in ideas or in facts, without a previous knowledge of the later developments of Arabic chemistry, especially in Spain; now it is of Spanish-Arabic alchemy that we know least. A work of capital importance here is the *Rutbatu'l-Hakim*, ascribed, probably falsely, to MASLAMA AL MAJRITI, who died in the early years of the eleventh century, and it is to be hoped that the task of editing, translating and annotating this valuable book will be undertaken at no distant date.

The *Liber de Septuaginta* is probably the most useful tool which BERTHELOT had at his disposal in this connexion, as it appears to be directly translated from the Arabic of JĀBIR IBN HAYYĀN. He deals with this in Chapter IX of Volume I, but it deserves a much fuller treatment. As an example of the superficiality of his work one may mention that, in comparing the list of titles of works of JĀBIR quoted in the *Liber LXX* with that given in the *Kitāb al-Fihrist*, he has overlooked the correspondence between the *Intentio Intentionum* of the former with the *Kitāb 'Ard al-'Irād* of the latter, and that between the *Albicalmon* and the *Kitāb Abi Qalamun*, the *Elementum yrci* and the *Kitāb Ustuqus al-'Uss al-Awwal*, the *Chapitre des luts* and the *Kitāb al-Tīn*, and others (26).

(26) I have not seen BERTHELOT's more recent edition of the Liber LXX, in *Archæologie*, 1906.

We have seen therefore :

1° That BERTHELOT had no idea of the vast extent of Arabic chemical literature ;

2° That he used the bibliographical material only superficially ;

3° That even in this superficial examination he was often very inaccurate ;

4° That many of his identifications are unsupported by evidence, and that others are certainly incorrect ;

5° That he took no pains to check the dates assigned by him to various authors, even ascribing AVICENNA, in one place, to the 12th century ;

6° That the texts he printed cannot be said to be properly established, since they were made from single MSS., even where other MSS. of the same works existed ;

7° That the works of « CRATES » and « AL-HABIB » appear to have been unknown to most of the chemists of Islam ;

8° That the works of JÂBIR chosen are not representative ;

9° That the absence of appropriate pagination in the texts and translation renders reference very difficult ;

10° That the annotations are few and chiefly concerned with one point only, namely, the relation between Greek and Arabic alchemy ;

11° That the use of mediæval Latin works to explain Arabic ones is very limited in its application, unless a previous knowledge of the latter has been obtained ; and

12° That the most important Latin work employed by BERTHELOT has received insufficient treatment.

The general impression conveyed by a study of Volumes I and III of *La Chimie au moyen âge* is, indeed, that of a confused mass of undigested material, rendered still more confused by careless scholarship. It is not surprising, therefore, that the conclusions at which BERTHELOT arrived are frequently misleading and very often quite inaccurate, while many others require considerable modification. It is to an examination of these conclusions that we must now turn.

(IV) BERTHELOT'S CONCLUSIONS ON ARABIC CHEMISTRY

Since BERTHELOT arrived at conclusions of the widest extent as to the nature of chemistry in Islam, one cannot hope here to deal with

the whole matter fully, or even, indeed, with more than a fraction of it. To examine it fully would be to write a complete history of Muhammadan chemistry, for BERTHELOT has something to say about the earliest period and the latest, in spite of the fact that he did not know what was going on in the meanwhile. I propose, therefore, to state shortly some of the principal points which BERTHELOT believed himself to have made, and to examine these briefly in order to arrive at some estimate of their worth. This is all the more necessary since historians of chemistry have, up to quite recently, accepted BERTHELOT's work was as authoritative (27).

1. « Les Arabes, en effet, n'ont pas connu les Grecs directement, mais par l'intermédiaire des Syriens, qui avaient les premiers traduit les philosophes et les savants grecs dans une langue orientale. » (Vol. I, p. iv.) Comment upon this conclusion may be confined to the remark that it is an over-statement of the case, and that, as STEINSCHNEIDER has abundantly shown, numerous books were directly translated from the Greek into Arabic. One may also refer to the well-authenticated tradition that KHALID IBN YAZID had many Greek alchemical treatises translated into his mother-tongue. Whilst BERTHELOT has conclusively shown that much alchemy passed to Islam from the Greek *via* Syriac, he has under-rated the Persian contributions, which were, perhaps, as important as those of the Greeks themselves. Additional light on this subject is being provided by the masterly researches of Prof. RUSKA.

2. « L'ensemble des ouvrages arabes que nous allons publier, joints aux traités syriaques contenus dans un autre volume, présente un grand intérêt pour l'histoire de la science, attendu qu'il fournit une connaissance solide de la véritable alchimie arabe, ignorée jusqu'à ce jour. » (Vol. III, p. 25.) This passage requires the most careful examination, for it makes two definite claims, namely (*a*) that BERTHELOT's work was the first in the field, and (*b*) that it furnishes a sound knowledge of the real alchemy of the « Arabs », or rather

(27) Even de LACY O'LEARY has implicitly relied upon him in his *Arabic Thought and Its Place in History* (1922, p. 120). It is, however, to be noted that Prof. LYNN THORNDIKE (*History of Magic and Experimental Science*, 1923, II. 217), whilst accusing LIPPMANN of belittling BERTHELOT « on petty pretexts, » himself says « BERTHELOT is a poor guide in such matters (alchemical MSS. of the Middle Ages) since his pretentious volumes are based on the study of a comparatively small number of MSS. at Paris. »

Arabic-writing authors. The first of these two is merely a question of priority and of no particular importance, but it is necessary to point out that Prof. WIEDEMANN had been working at the subject for several years before BERTHELOT, and had published some of his researches, which appear to have been unknown to, or neglected by, the latter. KOPP also, although handicapped — like BERTHELOT himself — by lack of knowledge of Arabic, had investigated Muhammadan chemistry as far as possible in the circumstances, and had consulted Orientalists on several particular points. KOPP's work is still of the greatest value, and is characterised by its accuracy. Thus, while BERTHELOT was certainly the first to publish texts of Arabic chemical books, the work of the two men just mentioned should not be forgotten, especially as in many respects it reaches a much higher level of scholarship than does *La Chimie au moyen âge*.

The second claim (*b*), however, is really the crucial question of the whole discussion. In the present article much evidence has been adduced to enable us to arrive at a considered judgment on the matter, and the inadequacy of BERTHELOT's work has, I hope, been made clear. Nevertheless, in order to place the matter beyond doubt it may be advisable to consider more closely the proportion which exists between the fraction of the material used by BERTHELOT and the total field.

The period during which chemistry flourished in Islam covers some seven hundred years — from the time of KHALID IBN YAZID (seventh century) to that of AIDAMIR AL-JILDAKI (fourteenth century). The chief centres of study were Iraq and Spain, and in that order, JABIR IBN HAYYAN (eighth century) was undoubtedly the most accomplished chemist of the early period, and about 80 of his works are extant, whole or in part. The largest and most important is the *Kitab al-Khawass*, or *Book of Properties*, preserved in two MSS. in the British Museum. It will be obvious that a great deal of work will have to be done before a just estimate of JABIR's chemical knowledge can be obtained, and it will not be denied that BERTHELOT certainly was not in a position to make such an estimate. Several of his conclusions on this particular subject have already been proved incorrect, notably his statement that JABIR does not mention the sulphur-mercury theory of metals, nor admit « arsenic » as a third constituent of metals.

As far as our information goes, JABIR's death cannot be placed later than the reign of AL-MA'MUN (813-833 A. D.), so that it is apparent

that BERTHELOT has entirely neglected a period of no less than five hundred years, except for the short articles he has given upon BUBACAR (RHAZES) and AVICENNA, based on Latin MSS. and therefore not authoritative. His claim that his researches « furnish a sound knowledge of Arabic alchemy » is therefore absurd; even if he had modified it by adding the words « as far as it affected European alchemy », he would still have been claiming far too much, for the works of AL-RAZI and IBN SINA exercised a great influence upon the future development of the science. Moreover, since it was chiefly in the tenth to twelfth centuries in Muslim Spain that the transmission to Europe flourished, it is undeniable that a study of this period of Islamic chemistry is absolutely essential; and this BERTHELOT wholly neglected. The important « Letters » of the « Brethren of Purity » (tenth century A.D.), which contain a large section dealing with chemistry, and which were one of the chief sources of chemical knowledge in Moorish Spain, are not even mentioned, in spite of the fact that their plain and straight-forward style and notable freedom from mysticism bear the clearest witness to the fact that it was in Islam that chemistry first became a true science.

The numerous works of AIDAMIR AL-JILDAKI (died about 1360 A. D.), which form a sort of encyclopædia of chemistry and give one a more or less accurate picture of six hundred years of the rise and progress of the science in Islam, together with much biographical information and innumerable lengthy quotations, seem to have been quite unknown to BERTHELOT. AL-JILDAKI had a better knowledge of the Muhammadan chemists than anyone else had possessed, and he frequently gives long extracts from books which have now disappeared; he is in fact an indispensable source for the study of « l'alchimie arabe ». Fortunately, MSS. of his works are plentiful and copies may be found in all the great European libraries.

As a last source unused by BERTHELOT may be mentioned the large number of books on minerals and precious and semi-precious stones, which provide material of importance to both the chemist and the mineralogist. Further elaboration of this point seems unnecessary, but one may perhaps remark that, to look at the matter from a numerical standpoint, BERTHELOT employed less than 4 *per cent.* of the available material, dealt with only three important chemists (viz., JÂBIR, AL-RAZI and IBN SINA) out of, at least, two or three dozen who require study, and passed over in silence nearly half a millennium.

It is fitting that this article, which contains much criticism of BERTHELOT's work, should also draw attention to those features which are praiseworthy, and one may readily achnowledge that such features are many. BERTHELOT's courage in attacking a problem of so great a difficulty was amazing, and that he achieved some measure of success is not denied, even by those who have most strongly felt his deficiencies. After all, it must be remembered that these historical researches were only the product of the leisure of an extremely busy man, and as such they compel our admiration. It is a sufficient testimonial to the worth of the researches on Arabic chemistry, that every one who has worked at the subject since they were published has used them, and perhaps occasionally abused them, but has never been able to neglect them. I feel personally that their chief value lies in the stimulus they give, for no one can read them without realising that the history of the progress of chemistry in Islam is probably the most important question for the modern historian of science.

The proof of the debt which Islam owed to the Greeks, the elucidation of the great help rendered in this connexion by the Syrian translators, the publication of the *Liber LXX* and the nine works of JABIR IBN HAYYĀN, the translation of the passage from the *Fihrist*, the attention drawn to the important book of « BUBACAR » — for all these valuable contributions to knowledge BERTHELOT deserves our warmest thanks. The magnitude of his services was great and should not be belittled on insignificant grounds; yet I think they were greatest not in accomplishment, but in indicating what was to be accomplished. « *Ce n'est que le premier pas qui coûte* », and this was taken for us by BERTHELOT.

(*Bristol.*) E. J. HOLMYARD.

Medici e scienziati Arabi : Ali ìbn Ridwán

Abú l-Hasan... b. Giafar fu autodidatta e sostenitore del criterio che meglio s'impari sui libri anzichè dalla viva voce del maestro. Nacque in Gize d'Egitto verso l'anno 388/988, come pare si possa ritrarre dall' oroscopo ch'egli ha con gran precisione calcolato sulle tavole di Yahya. abi Mansúr e descritto in un frammento autobiografico conservatoci nel testo di Usaybiah e nella traduzione ebraica di Jehuda Charisi (principio del secolo 13° Cr.). Era figlio di un fornaio : a sei anni cominciò a studiare, a dieci venne al Cairo, a quindici iniziò lo studio della medicina e della filosofia; ma per l'esigua sostanza familiare con difficoltà potè avanzare nell'apprendimento della scienza, campando or con dar consigli astrologici per la strada (passo di al-Qifti, non chiaro), or con l'esercizio della medicina empirica e talvolta coll'insegnamento. Durò così perseverando nello studio fino all'età di 32 anni, quando il nome acquistatosi come abile medico gli procacciò agevolmente il necessario ed il superfluo.

Servì il Califfo fatimida al-Hakim abu Ali al-Mansúr 386-411H./996-1020 Cr., il quale lo nominò capo sugli altri medici; e crebbe sempre la sua fortuna, di cui usò con moderazione, con risparmio e a beneficio degli altri, essendo pio e caritatevole. Attese alla professione e all'insegnamento; ed avendo perduto in tenera età un figliuolo (mortogli pochi giorni dopo la nascita) e due figliolette (morte l'una a poco più di un anno, e l'altra dopo i sette), prese con sé nel tristo periodo della carestia e della pestilenza che infierì in Egitto fra il 445/1053 e il 447/1055 un'orfanella che gli crebbe in casa e fu, con la sua ingratitudine, cagione della sua rovina. Giacchè in capo a qualche tempo, avendo involato un ripostiglio d'oggetti preziosi e la somma di 20,000 dinár, risparmio del sobrio e laborioso medico, fuggì via e non se ne seppe mai più nulla; ciò fece dar di volta al cervello del vecchio Ibn Ridwân. Il quale morì di lì a poco, nel 453/1061, secondo riferisce Usaybiah, o verso il 460/1067, come s'esprime Qifti, sotto il Califfato di al-Mustansir.

La sua vita fu semplice, operosa e retta. Nella epistola autobiografica succitata e nel Commento al « Quadripartito » di Tolomeo, Ibn

Ridwân ci fornisce notizie interessanti sulla sua istruzione, le abitudini, le idee dominanti nel suo pensiero, i suoi studi prediletti. Scelse la Medicina fra tutte le varie discipline, come quella che è più prossima alla filosofia, che meglio rispondeva al suo attaccamento alla pietà ed alla virtù, e che del resto gli era destinata dall'oroscopo stesso della sua nascita. L'agiatezza e poi la ricchezza, guadagnate dopo lunghi anni d'indefesso lavoro, lo trovarono abituato alla sobrietà, all'economia e alla beneficenza. Quel che avanzava al modesto dispendio per l'approviggionamento della casa (aveva nel quartiere del Cairo Qasr al-shamà la sua abitazione, che Usaybiah vide ancora e fu abbattuta al suo tempo) in « cibo, bevanda, olio, miele e legna da ardere », egli ne faceva risparmio per la vecchiaia o lo adoprava a soccorrere gl'infelici e sollevare gli afflitti. Ogni giorno, compiuti i suoi doveri professionali con umiltà e dolcezza, occupava il resto del tempo in opere di pietà e di devozione « ricreandosi a contemplare il regno del cieli e della terra e dandone lode a Dio ». Leggeva un trattato etico di Aristotele, e studiavasi di seguirne mattina e sera i precetti, facendo alla fine del giorno un esame di tutte le sue azioni e sentimenti : rallegravasi del bene, del bello e dell'utile compiuto, pentivasi del male, o brutto o dannoso che per avventura avesse fatto, e proponeva a se stesso di non più ricadervi.

Nonostante queste belle abitudini, Ibn Ridwân, forse per effetto appunto degli studi solitari, aveva, nella conversazione o corrispondenza scientifica con i suoi colleghi, maniere rudi, impazienti di contradizione, e spirito polemico acerbo; ciò che gli attirò l'antipatia e la critica aspra del suo contemporaneo Ibn Butlân, il quale s'era recato in Egitto col desiderio di conoscer da vicino il *rais al-atibbà*; ma dopo acri discussioni tenute con lui n'era partito scontento e disilluso. Una delle questioni su cui i due chiari medici trovaronsi in assoluto disaccordo, era se la medicina si apprenda meglio dai libri o dalla viva voce del mestro. Ibn Butlân compose uno scritto per sostenere con sei argomenti principali (riassunti da Usaybiah) i vantaggi e la superiorità dello insegnamento verbale e personale, contro l'opinione di Ibn Ridwân, cui non risparmia i frizzi e il rimprovero di scarsa cultura nella scienza degli Antichi. Naturalmente Ibn Ridwân non tacque, e la polemica si prolungò. Altre controversie scientifiche sostene Ibn Ridwân relative a Hunayn B. Ishâq, ad Ali abu l-Farag b. al-Tayyib, abû Bakr Muha. b. Zakariyya al-Razi, ecc.

La cultura filosofica e scientifica di Ibn Ridwân era tutta fondata sui libri; dei quali non amava studiar molti, ma pochi e scelti : gli altri

vendeva a qualsiasi prezzo capitasse o, più di rado, depositava come inutili nella sua libreria. Egli stesso enumera i suoi autori e volumi prediletti : cinque opere di letteratura (*adab*); dieci di dottrine religiose (*sharà*); in medicina, Ippocrate e Galeno « e simili », quali Dioscoride, Rufo, Oribasio, Paolo d'Egina; il *Hàwi* del Razi; quattro libri di agricultura e di farmaceutica; come opere d'istruzione (per l'insegnamento? = *Kutub al ta'alim* = Lehrbücher?) l' « Almagesto » e il « Quadripartito » di Tolomeo; e finalmente di scritti dei filosofi speculativi (*al-àrifun*), Platone, Aristotile, Alessandro d'Afrodisia, Temistio, e Muh. al-Farábi.

Nonostante la sua predilezione per i libri e per l'autodidascalia, Ibn Ridwán ebbe numerosi discepoli, i quali non pare gli facessero molto onore, se si può prestar fede a quanto dice di uno di essi ibn Butlan citato da abu l-Farag. Qifti aggiunge ch'essi andavan ripetendo, come appresi dal maestro, princípi medici, astrologici e logici tali da destare il riso; ma aggiunge « se la tradizione è autentica ». In generale Qifti è poco benevolo verso Ibn Ridwán, cui tratta da filosofastro e scrittore da strapazzo. Ma Usaybiah, che ne ebbe ben più larga e diretta conoscenza, avendo avuto sotto gli occhi gli autografi di lui, ce ne fa, come vedemmo, ben altro ritratto. Egli riporta fra altro alcune sentenze o motti d'Ibn Ridwan, che mette conto di riassumere e di riprodurre, perchè valgono a colorir la figura e il pensiero dell'autore. « Il miglior modo di servir Dio — egli diceva, — consiste nel contemplare i suoi regni, cioè la natura, e lodarne chi li governa ». « Il medico, secondo l'opinione d'Ippocrate, è colui che raccoglie in sé sette qualità o requisiti : 1° sanità di corpo e di spirito; 2° compostezza e grazia nel portamento e nel vestire; 3° fedeltà e segretezza in rapporto alle infermità dei suoi clienti; 4° desiderio della loro guarigione, assai più che della mercede che ne ritrarrà; e sia disposto a curare i poveri piuttosto che i ricchi; 5° brama d'imparar sempre più e di riuscir utile agli uomini; 6° castigatezza nel linguagio, continenza nello sguardo e nel pensiero verso la donna e la roba altrui; 7° onestà e rettitudine, per cui non prescriva veleni, nè insegni a farne uso, non somministri abortivi, curi il suo nemico, come cura l'amico. » Ed aggiungeva; « Maestro dell'arte medica è colui che, dopo essersi perfezionato nella scienza, abbia fatto suoi i sudetti requisiti; discepolo o aspirante alla medicina sarà colui, la cui fisionomia dia indizio d'indole buona, di spirito sagace, che sia desideroso d'imparare, intenda prontamente e ritenga quanto abbia appreso ». « Organismo esente da difetti è l'organismo o corpo sano di cui ogni membro conserva la sua

virtú, cioè compia come deve la sua particolar funzione ». « Quando sei chiamato da un infermo, comincia dal somministrargli qualche sostanza innocua, finchè tu ne abbia fatta la diagnosi e possa curarlo. Far la diagnosi della malattia significa riconoscere da quale umore essa abbia origine, e poi in qual membro sia localizzata. Dopo curerai l'infermo ». « Per conoscere difetti di una persona, ti conviene esaminare innanzi tutto la forma delle membra, il colore, la fibra (o complesso), la sensibilità tattile della pelle; esplorare il funzionamento interno ed esterno degli organi, provarne l'udito con chiamar da lontano e da vicino, la vista con mostrare oggetti a varia distanza, la lingua facendolo parlare, l'energia muscolare, ordinandogli di sollevare dei pesi, prendere, stringere, camminare avanti e indietro; esplorerai lo stato dei visceri, facendolo sdraiare supino colle mani protese, il cuore dal polso e dalle passioni dominanti o costumi, il fegato dall'urina, l'intelligenza con varie domande, la docilità con vari ordini, i costumi dal veder quello che lo attira e lo commuove. Utilizzerai dunque, secondo il caso, il senso diretto o l'indizio precipuo o l'opportuna interrogazione, finchè tu abbia conosciuto se vi sia difetto presente o imminente, ovvero se l'organismo sia sano e integro ».

Bibliografia. — Ibn Usaybiah enumera 102 scritti di J. R.; dal qual numero bisognerà sottrarre una decina di titoli, che sono più o meno evidentemente doppioni gli uni degli altri. Riporteremo i titoli di maggior curiosità, o da cui si possa ritrarre qualche particolare biografico.

1° *Opere mediche :*

a) « Commenti (*sharq*) a vari libri di Galeno; uno dei quali, « sulle sette » fu terminato un giovedì, quando mandavano due giorni alla fine di Dzú l-Higgiah dell'anno 432/1041; quello sulla « Piccola arte » di G. fu tradotto una prima volta in ebraico da Samuel b. Tibbon, 1199 in Beziers, poi in latino da Gherardo di Cremona (stamp. 1496), novamente in ebraico da sulla versione latina da Hillel b. Samuel verso la fine del secolo 13°; l'altro sugli « Elementi », che sarebbe il primo fra tutti, fu anch'esso volto in ebraico da un anonimo;

b) « Principi o Elementi nella Medicina », tradotto due volte in ebraico da Kalonymos b. Kalonymos, la prima volta nel 1306, la seconda ad Arles nel 1307;

c) « Studio accurato e vasto sulle « Questioni » di Hunayn »;

d) « Metodo utile per insegnar la Medicina », in tre libri;

e) « Trattato dell' orzo e dell'uso che se ne fa », dedicato al medico ABU ZAKARIYYAH YAHÚD B. SA'ADAH;

f) « Risposta al alcune domande di IBN SAÁDAH sul latte d'asina »;

g) « Scuola o metodo di IPPOCRATE sull'insegnamento della medicina »;

h) « Squarci (*intaza'at*) dei Commenti di GALENO ai libri d'IPPOCRATE »;

i) « Libro della Vittoria (o superiorità : *intisár*) di ARISTOTELE, ovvero libro di mediazione tra lui e i suoi contraddittori intorno all' « Udito naturale »;

k) « Esposizione della *Legge medica* d'IPPOCRATE »;

l) « Esplicazione ecc. del Testamento d'IPPOCRATE, conosciuto col titolo di « Ordine o Serie della Medicina »;

m) Note (*taliq*) al libro del TAMIMI « Sugli alimenti e i medicamenti »;

n) Note sul libro di POSIDONIO intorno alle bevande gradevoli ai sani;

o) « Excerpta o « utilità » (*fawáid*) dall'opera di FILAGRIO sulle bevande utili e gradevoli nelle ore (o fasi) delle malattie »;

p) « Che ogni membro s'alimenta d'umori congeneri »;

q) « Del metodo per contare il numero delle febbri »;

r) « Risposte intorno al polso, a uno della Siria »;

s) « Degli umori », risposta allo SAYKH ABU L-TAYYIB AZHAR B. AL-NUMÁN;

t) « Risposta al consulto richiestogli per iscritto da ABU L-ASKAR AL-HUSAYN B. MADÁN MALIK MAKRÁN intorno a una sua emiplegia sinistra »;

u) Excerpta da parecchie opere di GALENO e d'IPPOCRATE;

v) « Risoluzione dei dubbi di AL-RÁZI intorno ai libri di GALENO : in 7 trattati »;

x) « Lettera ad ABU ZAKARIYYAH YAHÚD B. SAÁDAH intorno alla *Microtechne* di GALENO »;

y) « Questioni discusse fra lui e IBN AL-HAYTHAM intorno alla Via lattea ed al luogo donde essa esce »; se ne conosceva al tempo di 'USAYBIAH solo una porzione del principio;

z) « Dei semplici » in ordine alfabetico trattati, di cui si conservavano soli i primi 5 e parte del 6° »;

a[1]) « Di quel che si deve trovare nel gabinetto del medico »; 4 trattati;

b[1]) Polemica contro IBN BUTLÁN : Confutazione dello scritto di I. B. « sul pollo ed il pulcino »;

c[1]) « Sulle stupidaggini di I. B. »;

d[1]) « Quel che I. B. ignora è certezza e saggezza; quel che egli sa è errore ed ignoranza »;

e[1]) « Che I. B. non conosce parola sua oltre la parola altrui »;

f[1]) Lettera ai medici d'Egitto e del Cairo sul conto di I. B. che termina, nel codice di Leida 1334 : « E questo basta perchè voi vi stupiate di codesto uomo, e ne ridiate, e tronchiate con lui ogni discorso appena cominciato, e non badiate a cosa ch'ei vi dica, ma in conto lo abbiate di un pazzo tignoso. »

Tra autobiografico, medico e polemico, sembra essere stato lo scritto che appare sotto vari titoli nel Catalogo di Usaybiah : « Intorno alla sua vita » :

« Del tendere sulla via della Medicina verso la felicità »;

« Intorno alla via della felicità, ossia vita ch'egli s'elesse »:

« Memoriale intitolato la perfezione perfetta e la felicità estrema non perfetta. » Questa sarà la *Tadzkirah* o « Memoriale », che negli estratti autobiografici riportati da Usaybiah, Ibn Ridwán dice componesse da 32 a 59 anni, rimaneggiandola e raffazzonandola ogni nuovo anno, introducendovi cioè probabilmente, le vicende ulteriori della sua vita o i frutti della sua nuova esperienza. Fu tradotta in ebraico da Jehuda Charisi.

II° *Scritti di scienza naturale, matematica e astrologica :* « Del topo »;

« Trattato di esortazione (o avvertimento) intorno alle fatiche di chi pratica l'arte dei giudizi delle stelle (o astrologia), e la nobiltà di quella gente (cioè degli astrologi) »;

« Nota che l'equatore abbia naturalmente notte più oscura, e che la sua sostanza sia per accidente più oscura di notte (?). »

III° *Etica :* Esplicazione, o versione del trattato del saggio Pitagora intorno alla virtù ;

Sul guadagno di un frutto, o interesse, lecito (ritratto) dal danaro;

« Di ogni politica »;

« Epistola sulla felicità »;

Note agli Excerpta dalle opere di Platone.

IV° *Filosofia :* Sulla sofistica di Abdallah b. al-Tayyib : « Immortalità dell'anima »;

« Sull'immortalità dell'anima secondo l'opinione di Platone e di Aristotele »;

« L'eccellenza della filosofia »;

« Riposta ad alcune questioni logiche del *K. al-qiyás* (le Analitiche) »;

« Della confusione tra il necessario e il contingente (propriamente, l'esistente) »;

« Del divenire del mondo »;

« Sulla fede monoteistica e la devozione religiosa dei filosofi »;

« Dell'uso della logica nelle scienze e nelle arti »;

« Note ed excerpta dall' *Isagoge* di Porfirio »;

« Piccola epistola sulla materia » : ad abu Sulayman b. Babisádz.

V° *Teologia* : « Replica ad Efraím e ad Ibn Zarah sulla divergenza nelle religioni professate »;

« Sulla missione profetica di Maometto secondo la Bibbia e la filosofia »;

« Scusa per ciò in cui egli contradiceva ai tradizionisti »;

« Confutazione di Razi in rapporto alla Teologia ed alla missione degli Apostoli. »

VI° Di dubbio o indecifrabile contenuto sono parecchie altre opere, di cui abbiamo i soli titoli.

Ad Usaybia è sfuggita la menzione di uno scritto assai noto d'I. R. : il Commento al *Quadripartito* o *Tetrabiblos* di Tolomeo ; Commento (tradatto in latino da Aegidius de Tebaldis, e publicato col « Quadripartitum » a Venezia 1493, 1519) ricordato da Qifti, il quale lo giudica di non gran valore. Qifti menziona anche un'opera veduta da lui stesso intorno alla serie delle opere mediche di Galeno, e sul modo di studiarle, ove toccò l'opinione degli Alessandrini, che sarebbe, presso Us., « Libro delle utilità nell'arte dell'insegnamento della Medicina ».

Per altre opere che i manoscritti conservano e attribuiscono ad I. R. vedi : Steinschneider nelle dotte note alla vita del Baldi ; di Ali Abenrodano, nel *Bullettino di bibliografia e di storia delle scienze matematiche e fisiche*, tom. 5°, 1857.

Ibn Ridwán. — dice al-Qifti — scriveva con un « carattere medio ». Aveva copiato per suo uso il trattato di Ibn Haytham « sulla luce della luna ». Qifti vide questa copia, datata di venerdì a mezzo Shabán 422, e notò che le figure matematiche ne erano belle e giuste e dimostravano buona pratica.

Fonti : usaybiah, II, 99-105.

Qifti, 294, 298 e segg., 443-444.

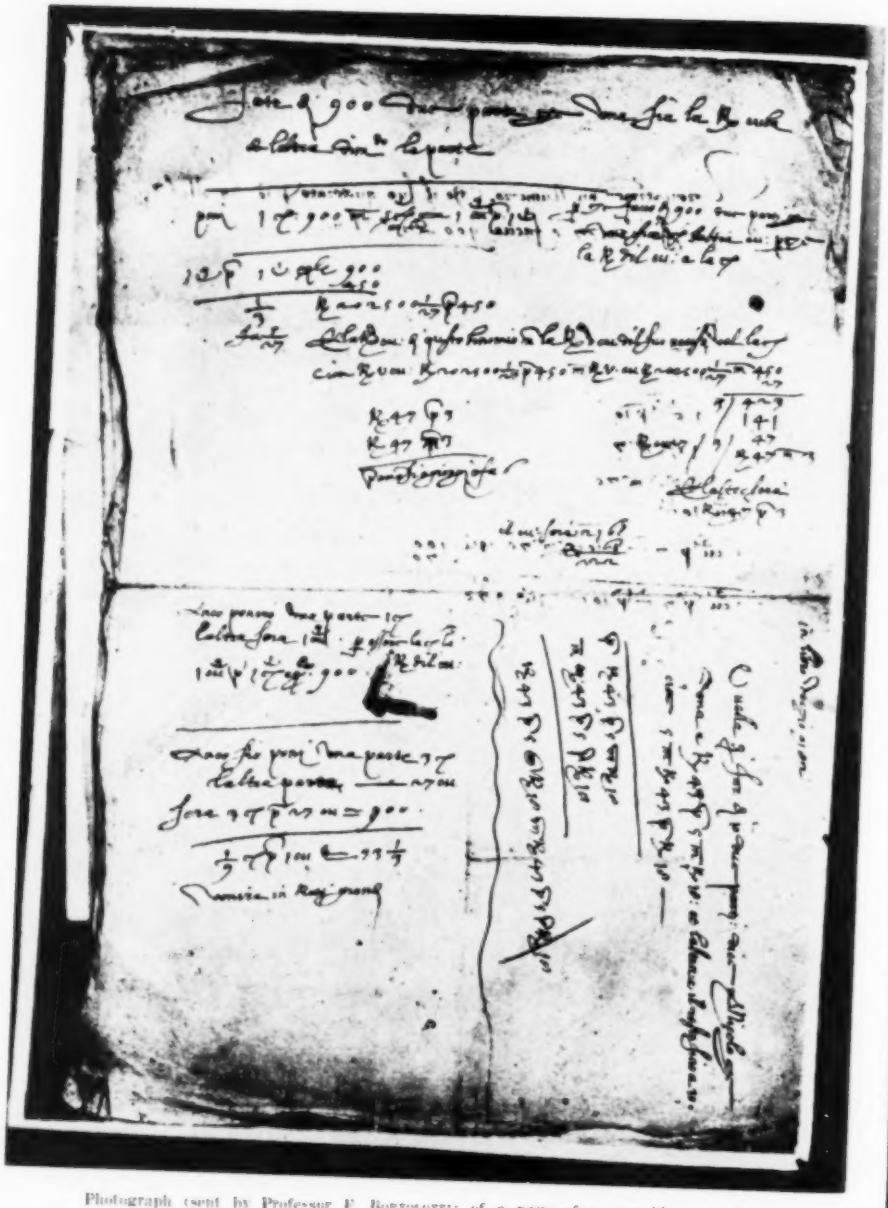
Steinschneider, *Pol. und Apol Litt.*, p 96-98; Farabi (*Mémoires de l'Acad. Imp. des Sciences de Saint-Pétersbourg*, série 7°, t. XIII, 1869), p. 37, 62, 119, 131, 134, 163, 167, 170-171, 175; — *Die hebr. Uebersetz*, p. 354, 733-735, 525-526; *Vite di matematici Arabi*, di B. Baldi, p. 40-55 dell'estratto.

Suter, *Mathematiker und Astronomer*, no. 232.

Leclerc, I, 525-530.

Brokelmann, I, 484.

(Roma.) Giuseppe Gabrieli.



Photograph (sent by Professor E. Bortolotti) of a page of a pamphlet containing studies and notes taken from the lessons of Pompeo Bolognetti (Bologna, ? — 1568). This manuscript pamphlet is found in the Library of the University of Bologna and is catalogued as No. 595. — N.

Note on our sign of Equality.

As a supplement to my article on Signs of Equality in *Isis*, vol. 5, p. 116-125, I herewith present some data and a photograph sent to me by Professor E. Bortolotti, which show that the sign = for equality occurs in a manuscript in the library of the University of Bologna, written before 1568 and possibly before 1557 (the date of publication of Recorde's algebra in which this symbol first occurs in print). Our photograph exhibits one page of a manuscript consisting of studies and notes to the lessons of Pompeo Bolognetti who died in 1568. The problem treated is to divide 900 into two parts, one of which is the cube root of the other. The smaller part is designated by a symbol consisting of *c* and a flourish (probably intended for « co »). Then follows the equation $900 \bar{\text{m}}\ 1\ \text{co}^{\text{①}} = 1\ \text{cu}^{\text{③}}$ (our $900 - x = x^3$). Here one sees a mixture of two notations for x and x^3, the notation « co » and « cu », made familiar by Luca Pacioli, and the exponential notation, with the 1 and 3 placed above the line, each exponent resting in a cup. This exponential notation has become familiar to historians through Bombelli's algebra, printed in 1572. Bortolotti informs us that his algebra existed in manuscript for twenty years before it was printed (1).

Therefore it is possible that the part here photographed may go back as far as about 1550. The cross-writing in the photograph begins : « in libro vecchio a carte 82 : quella di far di 10 due parti : dice messer Nicolo che l'ona e R 43 p̄ 5 m̄ R 18 : et l'altra il resto sino a 10, cioe 5 m̄ R 43 p̄ R 18 ». This Nicolo is supposed to be the Nicolo Tartaglia who died in 1557. The phrasing « Messer Nicolo » implies, so Bortolotti argues, that Nicolo was a living contemporary. If these contentions are valid, then the manuscript in question was written in 1557 or earlier.

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(1) A study of manuscripts relating to Bombelli and his algebra is published by E. Bortolotti in « Scientia », vol. 33, 1923, p. 385-394.

The question as to the origin of the Italian = is problematical. In the fifteenth century, REGIOMONTANUS in his letters to GIOVANNI BIANCHINI, expressed equality by a dash —. The Florentine F. GHALIGAI in his « Pratica d'arithmetica » of 1552 (earlier editions 1521, 1548) uses the dash. As the dash had other significations, a repetition of the symbol, thus =, may have been introduced to avoid ambiguity. In our photograph equality is expressed in two or three ways, namely by =, by « egle » (equale), and finally (in the lower left hand part) by a dash, unless indeed this dash in « — 27 cu » is merely to guide the reader to the « 27 cu », written further to the right, below the « 3 co ».

There is no indication that there was any intercommunication between RECORDE and the mathematicians of Bologna, either direct or indirect. RECORDE chose = because « noe. 2. thynges, can be moare equalle. » We have here the extraordinary spectacle of the same arbitrary sign having been chosen by independent workers guided in their selection by different considerations.

University of California. FLORIAN CAJORI.

Zur Geschichte der gebrochenen Exponenten

I.

Will man sich über das erste Auftreten und die weitere Entwickelung der gebrochenen Exponenten unterrichten, so stösst man bei CANTOR (1) und in der ersten Auflage von J. TROPFKE's « Geschichte der Elementar-Mathematik » (2) lediglich auf die Ergebnisse der ersten darauf bezüglichen Untersuchung von M. CURTZE (3) aus dem Jahre 1868. Im Winter 1920-1921 unterzog ich die Ergebnisse CURTZE's einer Nachprüfung, indem ich sie gleichzeitig zu ergänzen suchte. Die Haupttatsachen trug ich Ostern 1921 auf einer Versammlung in Göttingen vor. Veröffentlicht wurde aber nur eine kurze Notiz (4) und es gelang noch, die Darstellung in der zweiten Auflage von TROPFKE's Werk zu berichtigen (5). Mit dem vorliegenden Aufsatz will ich meinen eigenen Befund etwas ausführlicher in begründeter Form bekannt geben.

Durch die Veröffentlichung der Thorner Handschrift R. 4°. 2. wurden die Historiker der Mathematik auf einen ganz vergessenen mittelalterlichen Gelehrten aufmerksam gemacht, der seinerzeit im Rufe eines guten Mathematikers (was man damals so nennen konnte) gestanden hatte (6) : NICOLE ORESME, gestorben als Bischof von Lisieux (bei Le Hâvre) am 11. Juli 1382. Es waren zwei Abhandlungen von

(1) M. CANTOR, « Vorlesungen über Mathematik », 2. Bd., 2. Aufl., Leipzig 1900, S. 133-137.

(2) 1. Bd., Leipzig 1902, S. 206-207.

(3) *a.* « Der Algorismus proportionum des NICOLAUS ORESME ». Zum erstenmale nach der Lesart der Handschrift R. 4°. 2. der Kgl. Gymnasial-Bibliothek zu Thorn herausgegeben von E. L. W. M. CURTZE. 30 S. 8° mit 17 Fig. auf einer lithogr. Tafel. Berlin, 1868. — *b. Zeitschrift f. Mathematik und Phys.* 13. 1868, Suppl., S. 65-79 und 101-104.

(4) *Unterrichtsbl. f. Math. u. Naturw.* XXVII. 1921, S. 59.

(5) 2. Aufl., 2. Bd. Allgemeine Arithmetik. Berlin 1921. S. 132-134.

(6) Vgl. ALVARUS THOMAS, « Liber de triplici motu », Paris 1509, Bl. e ii, r°, Sp. 1 : Sed saluo meliori iudicio et auctoritate tam circumaspecti viri signanter in mathematicis scientiis.

ihm, die damals wieder ans Licht gezogen wurden, der « Tractatus de latitudinibus formarum » und der « Algorismus proportionum ». Im ersten fand man eine merkwürdige, wenn auch etwas primitive graphische Darstellung gewisser « Formen » (Wärme, Geschwindigkeit) von der man sich bald überzeugte, dass sie bis ins 17. Jahrh. hinein einen Lehrgegenstand an allen Universitäten gebildet hatte, im zweiten entdeckte man mit Staunen ein ausgebildetes Rechnen mit gebrochenen Potenzen. Mit der ersten Abhandlung beschäftigte ich mich bereits früher ausführlich (7), die zweite steht hier in Rede.

Die Veröffentlichung des « Algorismus » durch CURTZE musste sehr rasch erfolgen und war ausserdem seine erste derartige Arbeit. Sie ist also auch rein technisch nicht einwandfrei, und CURTZE hat selbst bereits einige Lesefehler in der angeführten Beschreibung der ganzen Handschrift (3*b*) korrigiert. Vor allem hat aber CURTZE eine Einteilung nach Nummern, Regeln usw. dazu gefügt, ohne das ausdrücklich zu sagen. Glücklicherweise ist der Buchausgabe ein Faksimile der ersten Seite der Handschrift beigefügt, sodass man sehen kann, welche Freiheiten sich CURTZE erlaubt hatte. Da ich den mir zur Verfügung gestellten Raum nicht ungebührlich in Anspruch nehmen will, verzichte ich auf eine Wiedergabe der « Regeln », die CURTZE in der Buchausgabe in der Zahl von 18 (in dem Aufsatz 19) aus den langatmigen Ausführungen ORESME's herauslas und bestätige nur, dass es sich in der Tat um ein vollkommen verstandenes Rechnen mit Bruchpotenzen handelt. Natürlich ist alles in fortlaufendem Text geschrieben, wenn auch zum Teil mit allgemeinen Buchstabengrössen. Die Regeln wurden übrigens von TROPFKE (1. Aufl.) in der CURTZE'schen Reihenfolge (nicht Numerierung) unter Weglassung von 17) und 18) wiedergegeben. Ich habe dazu folgendes zu bemerken.

ORESME hat keine eigentliche Grundzahl. Für ihn handelt es sich in der antiken, theoretischen Weise um ein Rechnen mit Potenzen von Verhältnissen (= proportiones). Daher der Titel. Dies ist jedoch

(7) *Bibliotheca math.* (3) 13, 1912/13, S. 115-145, ebenda (3) 14, 1913/14, S. 193-243. Ich stellte fest, dass man nicht von Koordinatengeometrie sprechen dürfe, da der Abszissenbegriff völlig fehlt, noch weniger von analytischer Geometrie (insbesondere auch gegen P. DUHEM), da von Analysis gar keine Rede ist, ferner, dass der Tractatus in dieser Form gar nicht von ORESME selbst stammt. In meinen Aufsätzen ist alles Nähere, vor allem die Literatur, zu finden.

nur eine Formsache, und es ist durchaus berechtigt, wie CURTZE das tat, die Regeln nach heutigem Gebrauche zu schreiben, z. B.

$$\frac{b^{\frac{1}{n}}}{a} = \left(\frac{b}{a^n}\right)^{\frac{1}{n}}.$$

Die ersten zwei Regeln lauten bei CURTZE

$$1)\quad a^m . a^n = a^{m+n}, \qquad 2)\quad \frac{a^m}{a^n} = a^{m-n}.$$

Diese Regeln kommen aber bei ORESME nicht ausdrücklich vor. CURTZE hat sich da verlesen. Die betreffenden Absätze haben bei ORESME die Ueberschriften : « Proportionem rationalem proportioni rationali addere » und « Proportionem rationalem a proportione rationali subtrahere » d. h. « Ein (rationales) Verhältnis mit einem ebensolchen zu multiplizieren, bzw. durch ein solches zu dividieren » (8). Die Absätze enthalten auch nichts anderes. Im ersten ist das Beispiel gegeben $\frac{4}{3} \cdot \frac{5}{1} = 6\frac{2}{3}$, im zweiten das Beispiel $\frac{3}{2} : \frac{4}{3} = \frac{9}{8}$.

Im übrigen bezweifle ich nicht, dass ORESME diese beiden Regeln 1) und 2) kannte und anwenden konnte ; denn die von CURTZE als 17) und 18) bezeichneten Regeln

$$17)\quad a^m . a^{\frac{1}{n}} = a^{m+\frac{1}{n}}, \qquad 18)\quad \frac{a^m}{a^{\frac{1}{n}}} = a^{m-\frac{1}{n}}$$

kommen in der Tat implizit vor. Die Regel 1) oder besser 2) wurde ja auch schon von EUKLID (IX, 11 und Porisma) in etwas anderer Form bestimmt ausgesprochen, und ORESME zitiert anderswo ausdrücklich das IX. Buch EUKLIDS (9). Die grosse Zahl von « Regeln » zeigt doch schon, auch ohne nähere Kenntnisnahme, dass ORESME ziemlich planlos Beispiele machte und diese offenbar nach dem Gefühl richtig behandelte.

Eine weitere Flüchtigkeit ist CURTZE ferner bei der Wiedergabe der ORESMEschen Schreibweise unterlaufen. Er gibt an (auch in dem Abdruck der faksimilierten Seite), ORESME habe die « proportio sesquialtera » $\left(=\frac{3}{2}\right)$ auch geschrieben $1^p\frac{1}{2}$. Im Original steht aber nur $\frac{p1}{1.2}$. Ebenso schreibt er für « quarta pars duplae sesquialterae

(8) Wegen der lateinischen Bezeichnungsweise siehe weiter unten.

(9) Im « Tractatus proportionum » (17).

(10) Die Punkte habe ich dorthin gesetzt, wo sie unbedingt nötig sind. Der Schreiber hat das nicht folgerichtig getan. Es sind natürlich nur Trennungspunkte.

[sc. proportionis] », also für $(2^{\frac{1}{2}})^{\frac{1}{2}}$, $\frac{1}{4}\,2^p\,\frac{1}{2}$. Im Original steht aber ganz deutlich $\frac{1\,p\,1}{4.\,2.\,2.}$ (10). Die rechteckige Umrahmung des Originals lasse ich weg, da sie offenbar nur gesetzt wurde, um die Ziffern gegen den Text besser abzugrenzen (auch bei einfachen Brüchen $\frac{1}{3}$, $\frac{1}{2}$, $\frac{2}{3}$). In seinem Aufsatz (3*b*) gibt Curtze ebenfalls beide Schreibarten an. Aber in dem Auszug am Schlusse des Aufsatzes ist der Abdruck ganz korrekt, und zwar genau so auch bei späteren Beispielen, die ich selbst nicht im Original gesehen habe.

Um die Oresmesche Schreibweise zu erläutern, hat sich nun Cantor ein Beispiel selbst zurecht gemacht und behauptet « Heute schreibt man $8 = 4^{1\frac{1}{2}}$, Oresme schrieb

$$\boxed{4^{p\,\frac{1}{2}}}\,4 \qquad \text{oder} \qquad \boxed{\begin{array}{c|c} p & 1 \\ \hline 1 & 2 \end{array}}\,4. \text{ »}$$

Die linke Seite ist nun nach obigem ganz falsch, aber sichtlich auch die rechte. Käme das Beispiel bei Oresme vor, so würde es vermutlich geschrieben sein :

$$\frac{1\,p}{1.\,2.\,4} \quad \text{oder} \quad \frac{3p}{2.\,4}. \text{ (11)}$$

II.

Die Fragen, die sich nun hier erheben und die bisher unbeantwortet blieben (z. T. auch gar nicht aufgeworfen wurden), sind folgende : 1) Wie kam Oresme dazu, sich mit Bruchpotenzen von Verhältnissen zu befassen ? 2) Ist er wirklich der « Erfinder » dieses Begriffes und der dazu gehörigen Rechenregeln ? 3) Blieb die Kenntnis davon auf Oresme beschränkt oder erhielt sie grössere Verbreitung ? Um diese Fragen zu beantworten, habe ich alle wichtigeren Schriften der damaligen Zeit vorgenommen, und diese mühevolle Tätigkeit ist auch nicht ohne Früchte gewesen.

Die Lehre von den Verhältnissen erfuhr im 14. Jahrh. unzählige Darstellungen. Das hing damit zusammen, dass an den Universitäten Oxford und Paris die Betrachtungen über die schon erwähnten « latitudines formarum » aufgenommen worden waren, denen wohl Oresme selbst das geometrische Gewand verlieh. Ich muss wegen

(11) Das Beispiel hat D. E. Smith in seiner « History of Mathematics », Vol. I, Boston 1923, S. 239 genau nach Cantor wiedergegeben.

dieses Gegenstandes auf meine zitierten Arbeiten verweisen. Doch will ich wenigstens erwähnen, dass die Scholastiker auf diese Weise zu dem Satze kamen, der Weg, den ein Körper bei gleichförmig beschleunigter Bewegung zurücklegt, sei gleich dem Weg in derselben Zeit bei gleichförmiger Bewegung mit der mittleren Geschwindigkeit. Ja man war mit so einfachen Bewegungen nicht zufrieden, teilte die Zeit (Grundstrecke) in unendlich viele Teile, die nach einer geometrischen Reihe mit gegebenem Quotienten abnahmen, und liess gleichzeitig die Geschwindigkeit nach einem einfachen Gesetz sich verändern (12).

Es ist nicht erstaunlich, dass bei solchen Betrachtungen auch Potenzen von Verhältnissen auftraten. Solche hatte ja schon EUKLID ins Auge gefasst und schon er nannte (V, def. 9, 10) das Verhältnis $a^2 : b^2$ das « doppelte », $a^3 : b^3$ das « dreifache » u. s. w. von $a : b$ (13). In Verfolgung dieser Bezeichnungsweise sagte man dann für die Multiplikation zweier Verhältnisse « Addieren », für die Division « Subtrahieren ». Hierfür scheinen antike Quellen nicht vorzuliegen. Der lateinische Uebersetzer des ANARITIUS-Kommentars erklärt aber schon « multiplicatio » und « aggregatio » bei Proportionen als gleichbedeutend (14). JORDANUS NEMORARIUS († 1237 ?), spricht in seiner « Arithmetica » direkt vom Addieren und Subtrahieren von Verhältnissen im angegebenen Sinne (15). Der Exponent des Verhältnisses heisst nun « proportio proportionum ». Vor ORESME habe ich diese Bezeichnung nicht gefunden. Aber sie kommt bei ihm schon in dem « Tractatus proportionum » vor, der dem « Algorismus proportionum » vorausgeht (16). Diese Schrift, die im Gegensatz zum

(12) Vgl. meinen Aufsatz : « Zur Geschichte der unendlichen Reihen im christlichen Mittelalter ». *Biblioth. math.* (3) 14, 1913/14, S. 150-168.

(13) Λόγως διπλασίων, bzw. τριπλασίων (vgl. auch EUKLID VI, 19, 20, VIII, 11, 18 u. VIII, 12, 19), lateinisch proportio duplicata, bzw. triplicata (vgl. M. CURTZE « Anaritii in decem libros... Euclidis commentarii, Lips. 1899, S. 163-165). ANARITIUS ist der bekannte arabische Mathematiker und Astronom ANNAIRIZI, der um 922 n. Chr. starb. Der Uebersetzer war GERHARD VON CREMONA († 1187).

(14) Vgl. a. a. O. S. 183 die Erläuterung zu EUKLID VI, 23, wo ein zusammengesetztes Verhältnis (λόγος συγκείμενος) ohne weitere Erklärung benützt wird.

(15) Der Titel der ersten, von J. FABER STAPULENSIS besorgten Druckausgabe (Parhisij 1496) beginnt : « In hoc opere contenta. Arithmetica decem libris demonstrata... » Vgl. dort Bl. (b5) v°.

(16) Beide Schriften sind auf etwa 1360 zu datieren.

« Algorismus proportionum » schon früh gedruckt wurde (17), enthält die gewöhnliche Proportionslehre nach Euklid, Boëtius und Jordanus (18). Sie war Curtze zwar bekannt, wurde aber bisher noch nicht beachtet. Als Einleitung zum « Algorismus proportionum » ist sie jedoch von grossem Interesse. Hier sagt Oresme zum Beisp. (19), er betrachte, wie die anderen Autoren nur Verhältnisse > 1; denn jedem solchen entspreche eines < 1 (proportio minoris inaequalitatis). Es ist also gar nicht richtig, wie Curtze sagt, dass Verhältnisse < 1 nicht als « proportio » betrachtet wurden und « fractio » hiessen.

Schon hier steht ferner (20), dass nicht jedem beliebigen rationalen Verhältnis eine ebensolche Hälfte (p. subdupla) oder ein rationales Drittel (p. subtripla) entspreche, d. h. in unseren Worten, dass $\sqrt{a}$ und $\sqrt[3]{a}$ im allgemeinen nicht aufgeht. Ja, er dehnt den Begriff noch weiter aus und sagt, dass nicht zu irgend einem Verhältnis ein anderes gefunden werden könne, das zu ihm im Verhältnis (p. proportionum) $\frac{3}{2}$ oder $\frac{4}{3}$ u. s. w. stehe. Oresme will also zum Ausdruck bringen, dass $a^{\frac{3}{2}}$, $a^{\frac{4}{3}}$ u. s. w. in der Regel irrational sind. Was er meint, erkennt man deutlich an einem Beispiel, das Oresmes Kenntnisse in helles Licht setzt. Er sagt, man nehme alle Verhältnisse (> 1), die durch die Zahlen (Verhältnisse) 1 bis 100 gegeben seien, bestimmt deren Anzahl richtig als 4950 $\left(100 \cdot \frac{99}{2} = 50 \cdot 99\right)$ und behauptet, unter den 4950 « Verhältnissen der Verhältnisse » seien nur 25 rationale. Diese zählt er auch auf. Es sind 15 aus der Reihe 2, 4, 16, 32, 64 $\left(6 \cdot \frac{5}{2} = 15\right)$, 6 aus der Reihe 3, 9, 27, 81 $\left(4 \cdot \frac{3}{2} = 6\right)$, dann noch die 4 Paare 5, 25; 6, 36; 7, 49; 10, 100. Der Gedanke der gebrochenen « proportio proportionum » ist also schon hier deutlich zu erkennen.

Wir können aber noch weiter zurück gehen. Eine der verbreitetsten Proportionslehren im Mittelalter war die von Bradwardin (verfasst 1328) (21). Schon Bradwardin führt als Beispiele für eine « proportio

(17) Enthalten in dem Bande, dessen Titel beginnt « Questio de modalibus bassani politi. . . » (Venetijs 1505), Bl. Cr° bis (C 10) v°.

(18) Auch Campanus und Johannes de Muris sind zitiert.

(19) Bl. (C 6) v°.

(20) Bl. C 5 v°.

(21) Gedruckt in demselben Bande wie der « Tractatus » Oresmes, Bl. 9 r°-Bl. 16 v°. (Die Bogen A und B zu je 8 Blättern sind foliiert. Mit Bl. (C 17) r° hört die Foliierung auf).

irrationalis » an (22) die « medietas duplae proportionis, quae est proportio diametri ad costam » (d. i. $2\frac{1}{2} = \sqrt{2}$) und die « medietas sesquioctavae proportionis (d. i. $\sqrt{\frac{9}{8}}$). Weiteres ist bei BRADWARDIN nicht festzustellen. Doch finden wir bei ihm ausser EUKLID, ARISTOTELES, ALPHRAGANUS und dem Almagest noch « Ametus filius Josephi » mit « epistola sua de proportione » zitiert, allerdings in einer harmlosen Sache (23). Das ist der bekannte AḤMED B. JÛSUF aus dem Anfang des 10. Jahrh., der in der Tat über das V. Buch EUKLIDS schrieb (24).

Auch die in der Hauptsache philosophische Schrift des BASSANUS POLITUS habe ich durchgesehen. Ueber Verhältnisse enthält sie nur das Allergewöhnlichste. Doch ist dort noch « alkindus » mit einem Traktat « de proportione et proportionalitate » angeführt und auf ihn wird bezüglich der Bezeichnung Subtrahieren für das Dividieren von zwei Verhältnissen verwiesen (25). ALKINDÎ († um 874) war einer der bedeutendsten Muslime seiner Zeit. Seine Schrift über die Proportionslehre, die im 14. Jahrh. offenbar noch in lateinischer Uebersetzung vorlag, ist aber heute verschollen.

Wir dürfen freilich bei diesen Arabern keine neuen Entdeckungen erwarten. Sie zeigen nur den Weg, auf dem die Scholastiker zu ihren Kenntnissen aus der Antike kamen. Kennten wir eine Stelle aus dem griechischen Altertum selbst, wo nur eine Andeutung eines gebrochenen Exponenten vorkäme, so wäre klar, dass der Begriff von dort über die Araber ins christliche Mittelalter kam. Eine solche Stelle, und nur eine, ist bekannt. ARCHIMEDES gebraucht ganz gelegentlich den Ausdruck ἡμιόλιος λόγος, d. i. das $1\frac{1}{2}$ fache Verhältnis, was an der angegebenen Stelle soviel wie $a^{\frac{3}{2}}$ bedeutet (26), wenn wir durch a das Verhältnis bezeichnen. Es ist zweifellos, dass diese Bezeichnungsweise wenigstens in einigen der zahlreichen griechischen « Elemente der Arithmetik » (zwischen 300 vor und 400 n. Chr.) fortlebte, von denen PROKLOS spricht (27), von denen uns aber nichts

(22) Bl. 10 v°.

(23) Bl. 11 r°, Sp. 2.

(24) Vgl. M. CANTOR, *Biblioth. math.* 1888, S. 7-10 und den ANARITIUS-Kommentar, ed CURTZE (13), S. XXVII.

(25) A. a. O. Bl. 6 r° u. 7 v°.

(26) Siehe « Opera omnia », ed Heiberg, Bd. 1. Leipzig 1880, S. 240; vgl. auch Bd. 3. Leipzig 1881, S. 230 (in der neuen Auflage I, 1910, S. 214 u. III, 1915, S. 198).

(27) Vgl. E. HOPPE, « Mathematik und Astronomie im klassischen Altertum », Heidelberg 1911, S. 217.

mehr erhalten ist. Bis jetzt ist kein Grund, anzunehmen, dass es nicht Oresme selbst war, der die ins Mittelalter gelangten Keime zur Entfaltung brachte.

Wir kommen nun zu der Frage, ob die Kenntnis der gebrochenen Potenzen mit Oresme wieder gegraben wurde und erst, wie man heute (mit der schon eingangs erwähnten Ausnahme von Tropfkes 2. Auflage) noch überall liest, ganz schüchtern bei Simon Stevin (1585) wieder auftauchte (28). Diese Frage beschäftigte schon Curtze, und er stellte ausdrücklich fest, dass er gebrochene Exponenten bei Cardano und in « anderen Werken des Anfangs der Buchdruckerkunst » vergeblich gesucht habe. Trotzdem war das Erlöschen dieser Kenntnis nicht wahrscheinlich Hatte sich doch das nämliche auch bei den « latitudines formarum » herausgestellt. Es gibt vor allem schon mehr Handschriften des « Algorismus proportionum », als Curtze im ersten Augenblick bekannt waren (29). Dann muss man nur den scholastischen Unterrichts- und Literaturbetrieb kennen, um es für unmöglich zu halten, dass eine solche, noch dazu von einem Manne wie Oresme stammende Neuerung nicht weiter beachtet worden wäre. Wenn man die Spuren aber in gedruckten Werken suchen will, muss man sich ebenfalls an die aus der Scholastik stammenden halten.

Da bot sich mir zuerst der i. J. 1509 zu Paris erschiene « Liber de triplici motu » des Portugiesen Alvarus Thomas dar, ein Buch, das P. Duhem und ich schon wegen der Reihenlehre im Mittelalter zu Rate gezogen hatten (6 u. 12). Dieses Werk sollte einen eingehenden Kommentar bilden zu den berümten « Calculationes » des sog. Suiseth, eines Engländers, der nach Oresme, aber noch im 14. Jahrh. schrieb, und das selbst nichts anderes ist als eine ermüdend weitschweifige und subtile Erörterung der Aristotelischen Bewegungslehre. Wie weit diese Dinge aber noch in unsere Zeit hereinreichen, wird dadurch belegt, dass noch Leibniz in einem Brief an Wallis vom 29. März 1697 (30) den Wunsch nach einer Neuauflage der Suisethschen

(28) Tropfke, Gesch. d. Elem.-Math., 1. Bd., Leipz. 1902. S. 201; Cantor, Vorles., 2. Bd., 2. Aufl., Leipz. 1900. S. 626.

(29) Ich kann z. B. die Handschrift C 80 der Dresdener Bibliothek, die im Besitze Wildmann's gewesen war, und die Münchener mathematische Sammelhandschrift n° 14908 aus dem Kloster St. Emmeram zu Regensburg anführen, beide den Mathematikhistorikern wohlbekannt. Ferner hat A. A. Björnbo auf eine solche, jetzt in Florenz befindliche, aufmerksam gemacht (*Bibl. math.*, (3) 12, 1911/12, S. 98).

(30) Wallis, Opera III, Oxoniæ 1699, S. 627-674, im bes. S. 673.

« Calculationes » aussprach. Das Werk von THOMAS umfasst 280 zweispaltig eng gedruckte, mit vielen Abbreviaturen gespickte Seiten, von denen allein 53 der Proportionslehre gewidmet sind. Nachdem ich mich durch ein gutes Dutzend Seiten durchgequält hatte, liess mich eine Bemerkung aufhorchen (31) : Man müsse wohl unterscheiden, das Verhältnis 16 : 1 sei doppelt so gross als das Verhältnis 8 : 1 (d. h. $16 = 2 \cdot 8$), es verhalte sich aber zum letzteren wie 4 : 3 (d. h. $16 = 8^{\frac{4}{3}}$). Das müsse man genau beachten, wenn man den Calculator (d. i. SUISETH) richtig verstehen wolle. Und nun kommt ein ganzes Kapitel, das fünfte, in dem THOMAS scharfe Angriffe gegen BASSANUS POLITUS richtet, weil er in seinen Proportionen (s. o.) diesen Unterschied gar nicht begriffen habe. Diese Einführung in die « Calculationes » führe eher heraus als hinein. Dann folgt (32) « Capitulum sextum in quo agitur de proportionum proportione ». Der Ausdruck selbst war schon vorher gefallen. Das Kapitel wird am Schluss als besonders wichtig bezeichnet. Es enthält aber nur Diskussionen und Polemik mit einzelnen Beispielen (wie $16^{1\frac{3}{4}} = 128$). Eigentliche Erklärungen und Regeln fehlen. Die Bekanntschaft mit dem Gegenstand setzt THOMAS also bei den Lesern voraus. Gegen « nicholaus horen » (d. i. ORESME) polemisiert er auch, in theoretischer Hinsicht, wobei er das schon oben angeführte allgemeine Urteil über ihn ausspricht (6). Ich konnte zwar die von THOMAS zitierten Stellen des Calculators nicht genau nachprüfen, da in der mir zugänglichen Ausgabe (33) die Distinctiones und Conclusiones nicht numeriert waren. Aus der Art der Zitate und der Durchsicht von SUISETH selbst scheint aber hervorzugehen, dass dieser nur ganze Potenzen, und zwar nur Quadrate, von Verhältnissen verwendet hat.

Jedoch machte mich Herr E. RATH, Direktor der Landesbibliothek in Stuttgart, darauf aufmerksam, dass solche gebrochene Potenzen auch in dem « Liber proportionum » des Pariser Kanonikus und Universitätsprofessors HIERONYMUS DE HANGEST (34) vorkommen, das

(31) A. a. O. Bl. d. ii, r°, Spalte 2, oben : Nam sexdecupla excedit octuplam per proportionem duplam : et se habet ad illam in proportione sexquitertia.

(32) Bl. d4, r°.

(33) « Calculationes Suiseth anglici », Papie M. ccccLxxxviij. Die Ausgabe (ohne Ort und Jahr) von JOANNES DE CIPRO enthält gleichfalls keine Numerierung, wohl aber die von VICTOR TRINCAVELLUS (Venedig 1520).

(34) Geb. in Compiègne, † 1538 in Le Mans, bekannt als eifriger Gegner Luthers.

i. J. 1508 zu Paris (Parrhisijs) herauskam (35). Das Werk ist freilich noch seltener als die schon angeführten Wiegendrucke. Nicht einmal das Britische Museum besitzt es. Ich war so glücklich, das Stuttgarter Exemplar benützen zu dürfen. DE HANGEST gibt eine vollständige Darlegung der alten und mittelalterlichen Proportionslehre. Das vierte Buch hat die eigene Ueberschrift « De proportione proportionum » (36). Dieses Kapitel ist viel umfangreicher als ORESMES « Algorismus proportionum » und rein theoretisch, während bei ORESME etwa zwei Drittel den Anwendungen gewidmet sind. DE HANGEST gibt zwar eine Reihe von Definitionen und Sätzen, aber die ORESMESchen Regeln finden sich bei ihm nicht in der Vollständigkeit und Klarheit, wie bei ORESME selbst, den er übrigens nicht anführt. Aus den Erörterungen geht aber hervor, dass er sich nicht für den einzigen hielt, der sich damals mit diesem Gegenstande befasste, und der ihm völlig klar war. Ich führe als schwierigere Zahlenbeispiele an $2^{\frac{1}{3}} \cdot \frac{3}{2} = \left(\frac{27}{4}\right)^{\frac{1}{3}}$ und $\frac{3}{2} : 2^{\frac{1}{3}} = \left(\frac{27}{16}\right)^{\frac{1}{3}}$, ferner $2^{\frac{1}{3}} \cdot 3^{\frac{1}{3}} = 72^{\frac{1}{6}}$ und $3^{\frac{1}{3}} : 2^{\frac{1}{3}} = \left(\frac{9}{8}\right)^{\frac{1}{6}}$.

Der Uebergang von der Scholastik zur modernen Wissenschaft erfolgte auf keinem Gebiete unter völligem Bruch mit dem Gewesenen. Waren doch die grossen Neuerer, wie GALILEI, DESCARTES und andere, alle Schüler von Scholastikern gewesen. So ist es auch mit dem Württembergischen Augustinermönch MICHAEL STIFEL, der uns, nachdem er zur neuen Lehre übergegangen war, als ganz moderner Algebraiker entgegentritt. In seiner « Arithmetica integra » (Norimbergae 1544) hat das 6. Kapitel (37) die Ueberschrift « De Proportionibus & earum Algorithmo ». Später kommt sogar noch der genau nach ORESME (dessen Name aber nicht angeführt wird) gebildete Untertitel « Algorithmus proportionum » STIFEL « addiert » und « subtrahiert » hier Verhältnisse, dann geht er zum « Multiplizieren » über, zuerst mit ganzen Zahlen, gleich aber auch mit Brüchen (38). Er erklärt

(35) Das Werk hat einen Satzspiegel von ca. 20 × 13,5 cm., also Folioformat, ist aber in 8° hergestellt. Es hat genau $3\frac{1}{2}$ Bogen = 28 unnumerierte Blätter.

(36) Bl. c. ij. v° bis Bl. (c6) r°.

(37) bl. 47-55.

(38) E. HOPPE hat das Verdienst, hierauf zum erstenmal aufmerksam gemacht zu haben. (*Mitt. Math. Ges. Hamburg* Bd. 3, 1900, S. 420). Dies wurde jedoch leider übersehen, so dass es auch in TROPFKES 2. Auflage noch nicht berücksichtigt werden konnte. Gelegentlich meines Göttinger Vortrags i. J. 1921 wies mich Herr HOPPE persönlich darauf hin, und ich habe dann STIFEL's Ausführungen noch näher beleuchtet in den *Unterrichtsblättern f. Math. u. Naturw.* XXVIII, 1922, S. 63-64.

sofort, man habe das Verhältnis mit dem Zähler des Bruches zu « multiplizieren » (d. h. in unserer Sprache zu potenzieren) und dann die Wurzel auszuziehen, die der Nenner angibt. Als Beispiel dient $\left(\frac{81}{16}\right)^{\frac{3}{4}} = \frac{27}{8}$. STIFEL schreitet aber nun auch zur « Division » eines Verhältnisses fort (was für uns Wurzelziehung heisst) und er bestimmt folgerichtig, dass « Division » mit einem Bruch gleich der « Multiplikation » mit dem reziproken Bruch ist. Das Verhältnis $\frac{27}{8}$, durch $\frac{3}{4}$ « dividiert », gibt demgemäss $\frac{81}{16}$.

Aber STIFEL hat gesehen, dass hier noch eine zweite Umkehrung möglich ist, und diese schiebt er zwischen der « Multiplikation » und der « Division » eines Verhältnisses ein. Er zeigt da, wie man ein Verhältnis durch ein anderes Verhältnis « dividiert » d. h. in unserer Sprache, er sucht den Exponenten bzw. Logarithmus eines Verhältnisses, wenn ein anderes als Grundzahl dient. Er will z. B. $\frac{729}{64}$ durch $\frac{3}{2}$ dividieren. Zu diesem Zwecke « subtrahiert » er, so oft es geht, und erhält 6. Also hat er, wie wir sagen, die Exponentialgleichung $\left(\frac{3}{2}\right)^x = \frac{729}{64}$ gelöst. Geht die « Subtraktion » nicht auf, so hält er ein, sobald das Verhältnis sein « genus » ändert, d. h. sobald es < 1 wird, wenn es vorher > 1 war. Das Beispiel ist hier, $\frac{2187}{128}$ durch $\frac{27}{8}$ zu « dividieren ». Die erste « Division » ergibt $\frac{81}{16}$, die zweite $\frac{3}{2}$, die dritte $\frac{4}{9}$; die letztere war also nicht mehr erlaubt. Wir würden nun schreiben $\frac{2187}{128} : \left(\frac{27}{8}\right)^2 = \frac{3}{2}$ oder $\frac{2187}{128} = \left(\frac{27}{8}\right)^2 \cdot \frac{3}{2} = \left(\frac{27}{8}\right)^2 \cdot \left(\frac{27}{8}\right)^{\frac{1}{3}} = \left(\frac{27}{8}\right)^{2\frac{1}{3}}$, So hat STIFEL zweiffellos gerechnet, wenn er auch nur das Resultat $2\frac{1}{3}$ für den « Quotienten » mit einer wenig besagenden Erklärung angibt.

Die grosse Bedeutung des STIFELschen Fortschrittes ist wohl von niemand erkannt worden. Es ist ja überhaupt bei der grossen Verbreitung des STIFELschen Werkes auffallend, dass in dieser Richtung nicht weiter gearbeitet wurde. Für uns genügt es, gezeigt zu haben, dass der scholastische Samen nicht auf felsigen Grund gefallen war. Die Ausdrücke, « doppeltes », « halbes », « $\frac{3}{2}$-faches » Verhältnis, in dem Sinne von a^2, $a^{\frac{1}{2}}$, $a^{\frac{3}{2}}$, wenn a das Verhältnis bezeichnet, haben sich noch viel weiter fortgepflanzt. G. ENESTRÖM hat dafür mehrere Beispiele aus GALILEI, GREGORIUS A ST. VINCENTIO und NEWTON angeführt (39). Zu diesen Beispielen möchte ich noch KEPLER fügen, der

(39) *Bibliotheca mathematica* (3) 12, 1911/12, S. 180-181. Dort auch der Hinweis auf die Stelle bei ARCHIMEDES.

in seiner « Nova Stereometria doliorum vinariorum » (Lincii 1615) derartige Bezeichnungen durchweg verwendete (40). Diese Ausdrücke waren noch im 18. Jahrhundert gebräuchlich. So lässt sich auch hier eine stetige Linie vom Altertum über das Mittelalter fast bis zur Gegenwart ziehen.

(*Augsburg*) H. WIELEITNER.

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(40) Siehe die « Opera », ed. Frisch, Bd. 4, Francof. et Erlangæ 1863, S. 607-609, 623 624. An der letzteren Stelle ist prop. subsemitripla für $1 : \sqrt{3}$ verwendet, was in der Übersetzung der « Neuen Stereometrie der Fässer » durch R KLUG (Ostwalds Klassiker, Nr. 165, Leipz 1908, S. 116, wo mehrere solche Ausdrücke zusammengestellt sind, nicht erwähnt ist.

Notes and Correspondence

I. History of Science Society

edited by F. E. Brasch. — Department of Terrestrial Magnetism, Carnegie Institution, Washington D. C., U. S.

Statutes.

1. This Society shall be called The History of Science Society. Its purpose shall be to encourage and maintain an active interest in the history of science in general and of the various sciences in particular.

2. The officers shall be a president, two vice presidents, a secretary, a treasurer (who may also be an assistant secretary), and a committee of publication to consist of three members who at the same time may hold individually any of the other offices.

3. There shall be a Council consisting of the officers together with fifteen other members. The Council shall have general charge of the affairs of the Society.

4. The president, vice presidents, and five other members of the Council shall be elected by ballot at the annual meeting of each year. An official ballot shall be sent to each member at least one month prior to the annual meeting, and such ballots, if returned to the secretary in envelopes bearing the names of the voters, shall be counted at the annual meeting. Each ballot shall contain a name proposed by the Council for each vacancy, with blank spaces in which the voter may substitute other names. A majority of all votes cast either in person or by mail shall be necessary to election. In case of failure to receive a majority vote for any office, the members present shall choose by ballot between the two having the highest vote. The secretary, treasurer, and the members of the Committee of Publication shall be elected annually by the Council.

5. All ex-presidents of the Society shall be ex-officio permanent members of the Council. For the president and vice presidents the term of office shall be one year, or until the incumbent's successor is elected, and each shall be eligible for not more than one reelection unless at least one year has elapsed since his preceding term. In case of a vacancy in any office, the Council shall elect a member of the Society to fill the vacancy until the next annual meeting. The terms of the first five members of the Council (aside from the

officers) in order of alphabetical arrangement, shall expire on December 31, 1924; the second five on December 31, 1925; and the third five on December 31, 1926.

6. Persons elected by the Council as members shall be admitted upon payment, within sixty days of the date of election, of the first annual dues (five dollars). Libraries and other institutions of learning are eligible to membership without vote. The fees to be paid by bona fide residents of countries having currency temporarily depreciated to any considerable extent shall be adjusted as the Council may from time to time direct. A member failing to pay his dues within sixty days of notification shall be considered as having withdrawn. Members elected during the year of organization (1924) shall be designated as Foundation Members.

7. The Society shall publish a journal which shall be sent without charge to each member. This shall contain reports of the meeting and such other papers and articles as the Committee of Publication may select.

8. The annual meeting of the Society shall be held in December of each year, the precise date and time to be determined by the Council. Other meetings may be called as the Council may direct. The Council shall meet at the annual meeting of the Society and may be called together or consulted by mail at the president's option.

9. These statutes may be amended by a two-thirds vote, by letter or in person, of the members of the Council.

Officers and Council Members of The History of Science Society for the First Year (1924).

President : Dr. L. J. HENDERSON.

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Dr. FLORIAN CAJORI.

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This list contains the names and addresses of all of the members who had been elected and had paid their dues on 31st May 1924. It has been decided that every member whose application is received within the year of organization 1924, will be designated as Foundation Member as soon as he has been elected and has paid his dues for the first year. A second and final list of Foundation Members will appear next year.

It has been decided by the Council, in accordance with Statute 6, that bona fide persons and institutions of continental Europe, with the exception of the Netherlands, Switzerland and Sweden, will not be expected to pay more than sixty Belgian francs for their annual dues. They are requested to send their applications for membership to Mr. L. GUINET, 30, Av. H. de Brouckère, Auderghem-Bruxelles, Belgium. The final list of Foundation Members will include the names of those European members.

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The first annual meeting of The History of Science Society will take place in Washington, D. C. on December 27, 28 and 29 1924, together with the fourth meeting of section L (History of Science) of the American Association for the Advancement of Science. Every member is earnestly and warmly invited to attend it.

FREDERICK E. BRASCH.

II. Queries and Answers

edited by FREDERICK BARRY, Columbia University, New York.

1. **Aristotle's knowledge of acoustics.** — In J. C. POGGENDORFF, *Histoire de la Physique*, translated by E. BIBART and G. DE LA QUESNERIE, Paris, 1883, the following sentence occurs on page 487 : « Deux siècles après PYTHAGORE, ARISTOTE écrivit sur les sons, et fit preuve d'une connaissance exacte des faits, dont il est peut-être redevable aux Pythagoriciens. Il savait, par exemple, que dans les cordes de tension égale et dans les tuyaux, le nombre des vibrations est en raison inverse des longueurs, et que les sons sont produits par des vibrations qui passent des corps sonores à l'air, qui les transmet à notre oreille. »

In his *Harmonie Universelle*, part 2, 1637, MERSENNE has the following proposition : « Déterminer pourquoy une chorde touchée à vuide fait plusieurs sons en mesme temps. » His comment begins as follows : « Il semble qu'ARISTOTE a cogneu cette expérience. »

What are the sources for these statements concerning ARISTOTLE's knowledge ?

(*Brown University*.) R. C. ARCHIBALD.

2. **Leonardus Qualea, Venetian physician fl. 1460.** — The Latin MS. 10.264 of the French Bibliothèque Nationale, wherein P. DUHEM discovered an unknown fragment of ROGER BACON's *Opus Tertium*, contains among other things a treatise entitled : Compendium clari viri LEONARDI QUALEA, quod Astronomiam medicinalem nuncupari voluit ex multis Syrorum, Indorum, Arabum, Persarum, Egiptiorum, Grecorum et Latinorum voluminibus compilatum in facilitatem medicorum et infirmorum. The whole MS. was copied by A. DE BRUXELLA (ARNAUD DE BRUXELLES) between 1475 and 1492; this particular part in 1475.

This « Medical astronomy » is a sort of scientific propædeutics for medical men ; it deals not simply with astrology proper but also with

astronomy, cosmography, meteorology, etc. We learn from it that the author, LEONARDUS QUALEA (presumably LEONARDO QUAGLIA), was Venetian. The Astronomia medicinalis was composed before 1475 and after 1457, probably c. 1460, because it refers to the volcanic eruption of 1457, which increased the size of the Palara-Kaimenè islet (near Santorin, one of the Cyclades), as to a relatively recent event. Who was this LEONARDO QUALEA or QUAGLIA? I have been unable to find any information about him, but the very meagre amount already mentioned and which can be summarized as follows : Venetian physician, flourishing c. 1460, author, c. 1460, of an astrological introduction for the use of medical men, called Astronomia medicinalis.

References : P. DUHEM. Ce que l'on savait des Indes Occidentales avant CHRISTOPHE COLOMB. *Revue générale des sciences*, t. 19, pp. 402-406, 1908; Études sur LÉONARD DE VINCI, t. 2, 1909, pp. 322-327.

G. SARTON.

III. The Teaching of the History of Science and Personalia

edited by HARRY ELMER BARNES. (Smith College, Northampton, Mass., U. S.).

Syllabus of New Honors Course in Principles, History and Method of Science at the University of London. — The program of this course is printed below because of the general significance of the movement which is thus initiated. In all probability it will be widely imitated. Its organization has been due to the activity of Dr. CHARLES SINGER and his associates, who have done so much to promote the study of the history of science in England. (*Isis* VI, 406). Dr. SINGER writes of the program contemplated in the syllabus :

« I thought perhaps you might like to see the final form of our syllabus for an examination in the Principles, History and Method of Science. If passed by the Academic Council of London University, as I have no doubt will be the case, the examination will be open to all those who have taken a degree in science in London University after one year's study, or will be available as a subject for a London degree to any who have a degree in another university after two years study. »

The syllabus in its final form follows :

Candidates will take :

1. A paper on the *General History of Science* (A).

2. A paper on *either* the *History of the Physical and Mathematical Sciences in Modern Times* (B) *or* the *History of the Biological Sciences in Modern Times* (C)

3. A paper on the *Principles and Method of Science* (D).

4. A special paper on an approved subject or subjects selected by the candidate from any part or parts of the syllabus.

Note on the Historical Papers.

The student will be expected to appreciate both the character of the Problems as in the minds of the thinkers of the time and the general nature of the solutions attempted, illustrated by the simplest details.

The mention of names in the syllabus is to be regarded merely as a guide to the meaning of terms. The list of names is not intended to be exhaustive nor does the omission of any name imply that the work of its bearer is not of primary importance.

A. General History of Science.

I. *The Beginnings of Science in Early Civilizations.*

II. *The Greeks* : *a*) The Pre-Socratics ; *b*) Plato and Aristotle ; *c*) Thinkers of Later Greek, Hellenistic and Roman Imperial Times.

III. *The Middle Ages : a*) Early Middle Ages. Scientific Thought in Europe and the East ; *b*) Later Middle Ages. Scholasticism and Science.

IV. *The Renaissance : a*) Naturalism and the Rise of Anatomy ; *b*) Mathematics and New Astronomy before Galileo.

V. *The Seventeenth Century : a*) Philosophy and the Experimental Method : Bacon, Descartes ; *b*) Mathematics and the New Physical Synthesis : Galileo, Kepler, Newton ; *c*) Transition from Alchemy to Chemistry : Mayow, Boyle ; *d*) Biological Conceptions : Harvey, Malpighi, Ray ; *e*) Foundation of the Academies.

VI. *General Scientific Tendencies in the Eighteenth and Nineteenth Centuries.* Interrelation of Physical and Biological Sciences.

B. — History of physical and mathematical sciences in modern times.

I. *The rise of the academies. The eighteenth century* : *a*) The new instruments and their influence on scientific ideas. Air pump, thermometer, barometer and meteorological instruments, optical instruments, pendulum clock ; *b*) Analytical and projective geometry The infinitesimal calculus : *c*) Universal gravitation and the problem of celestial mechanics ; *d*) Theories of light ; *e*) General dynamics and principle of least action. *f*) Systematisation of chemical knowledge. Phlogiston ; *g*) Discovery and manipulation of gases ; *h*) « Non-confinable simple substances ». Electric and magnetic fluids, phlogiston, caloric, ether.

II. *The nineteenth century* : *a*) Rise and development of atomic theory. The electrical theory of matter and the structure of the

atom; *b*) The nature of heat and the conservation of energy; *c*) Wave theory of light and development of ether theories; *d*) Current electricity and electro-magnetism. AMPÈRE, OERSTED, FARADAY, CLERK MAXWELL; *e*) Electro-magnetic theory of light and Herzian waves; *f*) Chemistry of carbon compounds. Synthesis of organic substances; *g*) Theory of solution. Ionic theory and its implications; *h*) Spectroscopy and astrophysics; *i*) Refinements of celestial mechanics. Theories of the universe; *j*) Age of the Earth and geological principles; *k*) Evolution of geophysical theory. Air, Sea, Earth; *l*) Modern mathematical ideas; *m*) The steam engine and other mechanical inventions. The rise of scientific technology.

C. — HISTORY OF BIOLOGICAL SCIENCES IN MODERN TIMES.

I. *The rise of the academies. The eighteenth century*: *a*) Influence of invention of the microscope; *b*) Beginnings of systematic biology. *c*) Mechanics of the animal body; *d*) Respiration, circulation and the entry of chemistry into biology; *e*) Theories of the nature of disease.

II. *Organic evolution. The eighteenth century*: *a*) Theories of organic evolution. BUFFON, ERASMUS DARWIN, LAMARCK, CHARLES DARWIN, DE VRIES; *b*) Cell theory and cell structure. Histology and histogenesis; *c*) Biogenesis and abiogenesis. REDI, SPALLANZANI, PASTEUR. Origin of living things; *d*) Quantitative methods in physiology. Mechanism and vitalism; *e*) Enzymes. Hormones, nutrition and growth. The beginnings of biochemistry; *f*) Germ theory of disease. Toxins and antitoxins. Immunity. The study of epidemics; *g*) Embryology and the theory of recapitulation. The application of experimental methods; *h*) Classification of animals and plants as influenced by evolutionary theory; *i*) Physiology of the plant. Oecology and the balance of life. Symbiosis and parasitism; *j*) The animal in relation to its environment. Symbiosis, commensalism and parasitism; *k*) The geological record. Succession of living forms. Antiquity and descent of man; *l*) Mendelian inheritance and statistical study of biological phenomena; *m*) The physiology of the nervous system and its application to psychology.

D. — PRINCIPLES AND METHOD OF SCIENCE.

I. *Ideals of science.*

II. *The problem of knowledge.* The empirical, critical and chief current theories of knowledge.

III. *Inductive and deductive methods.* Axioms, postulates, definitions, hypothesis, probability, verification. Description and explanation. The special sciences; extent of their autonomy.

IV. *Conceptions of natural law.* Uniformity, causation (transient and immanent), necessity and contingency.

V. *Thought and its instruments.* Language, symbolism, problems of abstractions.

VI. *Prolegomena to mathematics*: *a*) Nature of number. Theory of measurement, mathematical continuity and its relation to perceptual experience; *b*) Fundamental concepts of geometry. Elementary treatment of spatio-temporal systems (theory of relativity, etc.). Euclidean and non-euclidean geometries; their relation to experience; *c*) Applied mathematics, including the elements of statistical method.

VII. *Prolegomena to the physical sciences.* The data of knowledge of the physical world. Sense-qualities and their relation (*a*) to perceptual objects (*b*) to entities assumed in physical theories (molecules, atoms, electrons, ether, etc.).

VIII *Prolegomena to the biological sciences.* Empirical characters which distinguish living from non-living substance. The concept of organism. Problem of nature of life: biogenesis, heredity, individual and racial development.

IX. *Prolegomena to psychological science.* Empirical characters of mental activity. General characteristics of cognition, feeling and conation. Theories of relation of mental and bodily activity.

Karl Sudhoff's Seventy-First Anniversary (Nov. 25, 1923). — Dr. SUDHOFF's seventieth anniversary was splendidly celebrated last year at Bad Steben during the annual meeting of the *Deutsche Gesellschaft für die Geschichte der Medizin und der Naturwissenschaften.* A fitting occasion indeed, since Dr. SUDHOFF was the real founder of that society a little more than twenty years ago (his preface to the first volume of their *Mitteilungen* is dated February 1902), and has remained its soul and its main driving power ever since. A bronze plaquette (20×12 cm.) designed by Prof. Dr. KLEINSCHMIDT of Leipzig was presented to him, also not less than two *Festschriften*, the one, forming the fifteenth volume of the *Archiv für Geschichte der Medizin*, the other entitled Essays on the History of Medicine edited by CHARLES SINGER and HENRY E. SIGERIST. Thus three *Festschriften* have already been dedicated to the great Master of medico-historical studies and this testifies eloquently to the devotion of his many pupils and friends all over the world.

This year's anniversary will be a much quieter one, but there is no reason why it should be less happy. *Isis* takes advantage of this quietness to bring her own offering to Dr. SUDHOFF and send him her every best wish. May his wonderful activity be continued for many more years and may these years be full of happiness and health as well as work. G. S.

Heidelberg. — A Professorship in the History of Science in the Orient. Professor JULIUS RUSKA, of the University of Heidelberg, has been recently appointed to a professorship on the « History of Science in the Orient », a professorship which is certainly unique. He will lecture this summer (1924) on the history of alchemy. His own speciality, indeed, is the study of Muslim chemistry and mineralogy and of Muslim and Mediaeval culture in general. Prof. RUSKA is not by any means a stranger to our readers. They have been able to read in our journal many of his valuable studies (*e. g.*, I, 268-71, 341-50; V, 26-50; 409-17) and many others published by him in other places have been analyzed or mentioned (*e. g.*, I, 266-7, 547, 760; II, 258, 259, 290; III, 352, 443-4, 456, 476-7; V, 505, 533.).

G. S.

Athens. — A new professorship in the History of Science at the University of Athens. A professorship in the history of science has been recently (April 1924 or before) established at the University of Athens. Professor MICHAEL STEPHANIDES (whose work has been analyzed in *Isis*, III, 430-3) has been selected as the first incumbent. This selection could have been foreseen. Let us hope that Mr. STEPHANIDES' lectures on Greek and Byzantine science will eventually be published.

Bonn. — Dr. FRIEDRICH DANNEMANN, whose activity is as well known abroad as in Germany and whose textbooks have often been reviewed in *Isis* (see II, 218; IV, 110, 563; V, 198; VI, 115) will give this summer (1924) a series of lectures at the universities of Bonn and Cologne. The object of both courses is the same : to explain the development of scientific and technical culture. They give a brief outline of this development from early Babylonian and Egyptian times to our own days.

Institut für Geschichte der Naturwissenschaft in Heidelberg. — Durch die Begründung dieses Instituts sind Pläne verwirklicht, die der Leiter des Instituts seit einem Vierteljahrhundert verfolgt und, durch keine Schicksalswendung und Hemmung beirrt, im Auge behalten hat.

Die Bayrische Akademie der Wissenschaften hat einst unter LEOPOLD VON RANKES Vorsitz den Gedanken gefasst und verwirklicht, die Geschichte der Wissenschaften in Deutschland darzustellen. Eine Reihe heute noch klassischer Werke ist aus dieser Anregung hervorgegangen. Inzwischen ist auch die Erforschung der allgemeinen Geschichte der Naturwissenschaften in die Weite und Tiefe gegangen. Eine wachsende Zahl von freiwilligen Arbeitern hat unsere Erkenntnis auf allen Gebieten gefördert. Seit mehr als zwanzig Jahren besteht

die von P. KAHLBAUM und K. SUDHOFF begründete Deutsche Gesellschaft für Geschichte der Medizin und Naturwissenschaft mit ihrer über alle Neuerscheinungen berichtenden Zeitschrift.

Alles, was in Deutschland erreicht wurde, ist das Verdienst einzelner opferbereiter Männer; keines Mediceers Güte und keine staatliche Ermunterung hat ihre Arbeit erleichtert. Nur ein einziges Universitätsinstitut in Deutschland ist der Pflege medizinhistorischer Studien gewidmet. Es hat unter Geheimrat K. SUDHOFFS Leitung glänzende Leistungen hervorgebracht. Für die Geschichte der Naturwissenschaften ist es dagegen bei Ansätzen und Plänen geblieben. Die Gegenwart mit ihrer unerhörten Not scheint weniger denn je dazu geeignet, bisher Versäumtes nachzuholen. Dennoch wollen wir es wagen, mit bescheidenen Mitteln einen Grund zn legen und im Vertrauen auf eigne Kraft und guten Willen den Bau zu beginnen. Was uns vorschwebt, ist eine Arbeitsstätte, an der man in erster Linie sich der Aufgabe unterziehen will, das Mittelalter vom Uebergang der griechischen Wissenschaft zu den Muslimen bis zur Erneuerung des wissenschaftlichen Denkens im Abendland, bis zu den grossen Begründern der modernen Naturwissenschaft zu erforschen, neue Quellen zugänglich zu machen und unerkannte Zusammenhänge klarzustellen. Aber nicht nur die Fortentwicklung der Naturwissenschaften, sondern auch ihre Gebundenheit an die jeweilige Gesamtlage der geistigen Kultur soll Gegenstand der Untersuchung sein.

In den Heidelberger Akten der von-Portheim-Stiftung soll die Möglichkeit geschaffen werden, ausser den aus dem Institut hervorgehenden Abhandlungen auch Arbeiten auswärtiger Forscher zu veröffentlichen, soweit es die verfügbaren Mittel gestatten.

Wir hoffen, die Bibliothek des Instituts durch Beschaffung von Werken über die Geschichte der Naturwissenschaften, alten Quellenwerken wie Neuerscheinungen, auf den notwendigen Stand bringen zu können. Was uns aber besonders am Herzen liegt, ist die Erwerbung von Handschriften-Photographien, deren Bearbeitung durch sprachkundige und sachverständige Gelehrte in den Räumen des Instituts vorgenommen werden könnte. An Arbeitsstoff wird es nie fehlen.

(*Heidelberg, im Januar 1924.*) JULIUS RUSKA.
VICTOR GOLDSCHMIDT.

IV. Museums and Collections.

National Museum of Engineering. (Washington D. C., 1924). — A very powerful movement is on foot to create in the United States a National Museum of Engineering, to become a part of the National Museum of Washington which is placed under the care of the Smith-

sonian Institution. The creation of branches in other American cities is contemplated. The full text of the original circular, received by me in February 1924, follows:

« The Congress of the United States founded in Washington in 1846 the Smithsonian Institution with a Board of Regents composed of the Vice President as Chancellor, the Chief Justice, three members of the Senate, three members of the House of Representatives and six citizens at large, « for the increase and diffusion of knowledge among men ». Some seven years later it established the National Museum to house for preservation objects of historical value which were the normal developments of the various governmental departements from exploration and research and from gifts. Beginning with departements of natural history, botany, geology, and mineralogy, as the character of the country changed from an agricultural and trading populace to a manufacturing nation departments of engineering and industry were added. Now that we have become the leading industrial nation of the world these latter departments have come to assume a major importance and in them may be seen many items of prime historic and scientific interest.

« Among these are the WHITNEY Cotton Gin, the STEVENS Screw Propeller, the BLANCHARD Lathe, the SHOALES typewriter, the MORSE telegraph, the BELL telephone, the EDISON phonograph and incandescent lamp, the LANGLEY aerodrome, the WRIGHT aeroplane. The collections, large as they are, do not represent American inventions of the first rank as fully as they should. From sheer lack of space proffered gifts have had to be declined, and some of these may be lost forever. To meet the requirements indicated an «Organizing Committee of 100 » leading citizens has been formed and of this Mr. F. A. Delano, Regent, Smithsonian Institution is Chairman, Mr. GEO. E. ROBERTS, V. P. National City Bank, New York is Treasurer and Mr. H. F. J. PORTER, M. E., Engineering Societies Building, 29 W. 39th St. is secretary. An expert (Professor CHARLES R. RICHARDS), is now abroad making a study of the great foreign museums from which plans will be drawn for a new building. In this much of the material now in the departments of engineering and industry of the present National Museum including thousands of items from the Patent Office and other governmental bureaus, together with many drawings and documents of inestimable value will be installed. To this collection will be added every relic of value that can be procured. Such gaps as may then remain are to be supplied by models faithfully built from authentic sources. This done, the National Museum of Engineering will afford a comprehensive survey of American research and invention.

« But American trophies in these fields are not to stand alone. Beside them are to be placed tools, machines, engines and apparatus devised in other lands. To models of American origin will be added models chosen in a thorough examination of the industrial museums of South Kensington, Paris, Berlin, Munich and other foreign cities. Then it will be feasible to set forth the evolution of electrical art, of photography, of textile manufacture, of chemical industry and so on around the full circle of engineering and invention. This accomplished, and when the best methods of arrangement and admin

istration are exemplified, the National Museum of Engineering is to serve as a pattern for branch museums of industrial art in New York, Chicago, and other leading cities of the Union. Between the parent institution and its offspring there is to be constant and vital co-operation through the American Association of Museums to which practically all the Museums in this Country and Canada belong and which has its headquarters in the Smithsonian Institution in Washington. Its president, Mr. CHAUNCEY J. HAMLIN, is closely identified with the Organizing Committee of the National Museum of Engineering. Its treasurer, Mr FREDERIC A. DELANO is Vice Chairman of the latter. Through this close association the branch museums and traveling exhibits will be developed to take the museums to the people.

« It is highly probable that each branch museum will stress its local arts and industries, and draw upon local friends for financial and administrative support. The Chicago Museum may be expected to trace the evolution of sowers and cultivators, harvesters and tractors. Minneapolis will probably present with emphasis the successive triumphs which have led to her great flour-mills of today. Detroit may exhibit self-propeled vehicles of ancient designs, duly linked to the latest and best models of this hour. Pittsburgh will undoubtedly excel in equipping her department of metallurgy. New York will probably be more general and historical. And so on throughout our sisterhood of cities, each with its special distinction in science at work. To aid in the foundation and growth of branch museums it is proposed, at a centre to be duly chosen, to produce replicas of models, at minimum cost, for general distribution. Here, also, will be prepared films of the highest merit to serve audiences far and near.

« Today our principal cities have museums of fine art, of natural history, and public libraries of utmost usefulness. To these centres of instruction and culture will thus be added Museums of Engineering and Industry, to mark our gratitude to inventors and researchers, and freely bestow their golden gifts on all the people. We may be well assured that thousands of our youth, rich in native talent, will find inspiration and vocational guidance in the collections thus being planned today.

« What will be the probable cost of such a program ? The Smithsonian Institution, through its secretary, Dr. CHARLES D. WALCOTT, will ask Congress to approve the assignment gratis of the site for the new building on its grounds. The proposed building will cost, it is estimated, four-and-a-half million dollars. Adequately to endow the work will cost, in all likelihood, three-and-a-half millions. To place replicas of original models in a round of branch museums will require the income of two millions. In all, therefore, a total fund of ten million dollars will be needed. Of this amount a sum exceeding one million dollars is already promised.

« For further information communications should be addressed to H. F. J. PORTER, Secretary, 29 W. 39th St., New York City. »

The National Museum of Engineering was incorporated in the District of Columbia on March 8, 1924, the incorporators being FREDERIC A. DELANO, H. A. GILLIS and H. F. J. PORTER. « The

objects of said corporation shall be to establish and maintain a permanent organization and home for commemorating the achievements and perpetuating the records of engineering and industrial progress; to erect and equip suitable buildings for offices and exhibition purposes; to cooperate with National and State Governments, Museums, societies, educational institutions, industrial and mercantile corporations, and the public; and generally to do any and all such acts and things as may be necessary and proper in carrying into effect the purposes of the corporation. »

On April 28, 1924 the following officers were elected: President, ELIHU THOMPSON, Lynn, Mass.; Vice-Presidents, GUY V. TRIPP, New York. L. H BAEKELAND, Yonkers. N. Y., JAS. A. FARRELL, New York; Treasurer, GEO. E. ROBERTS, New York; Secretary, H. F. J. PORTER.

It is hardly necessary to add that *Isis* is in sympathy with this movement and makes every wish for its success. Our only regret is that the scope of the Museum was narrowed to Engineering. The incorporators would have been wiser, if, following the example given by similar museums in other parts of the world (Paris, London, Haarlem, Munich) they had included the development of pure science as well as that of its applications within the field of their activities. On the contrary, their objects (as defined in their charter of incorporation and reprinted above) make it quite clear that they are anxious to restrict their interest to « engineering and industrial progress. » Only the fruits interest them, they will have nothing to do with the seeds. This is, I repeat, a very narrow, a very ungrateful and very disturbing attitude. GEORGE SARTON.

The Oxford Museum of the History of Science (1924). — Through the persevering labours of Mr R. T. GUNTHER, of Magdalen College, the extremely valuable collection of early scientific instruments acquired by Mr LEWIS EVANS has now found a permanent home in the Old Ashmolean Museum at Oxford. No more appropriate resting-place for this unique series of historic instruments could be found than the building erected by the University in the reign of CHARLES II, to receive the famous collections of ELIAS ASHMOLE, and to serve as a centre for scientific studies in Oxford. The collection is the fruit of thirty years' assiduous devotion on the part of Mr EVANS, and it is quite certain that, if it were dispersed, no such series could ever be got together again. A remarkable feature of the collection is its wealth in early astronomical and mathematical instruments, dated examples of which go back so far as the thirteenth century. For rather more than a year past the collection has been housed as a loan

in the Picture Gallery of the Bodleian Library; now, however, it passes as a gift to the University, and the expenses of its removal to its new quarters will be met by the generous gift of 1000 L. from the Goldsmiths' Company and of 250 L. from Sir JOHN R. FINDLAY, of Balliol College. Mr GUNTHER is to be heartily congratulated on the successful issue of his untiring efforts, in the face of some opposition, to ensure the addition of this most interesting series to the permanent treasures of the University. (*Nature*, March 15, 1924, vol. 113, p. 400.)

The Harvard Museum of the History of Science. — This Museum does not yet exist except in the imagination of a few enthusiasts, but the success of Mr R. T. GUNTHER's efforts (1) gives them some hope that they, too, may succeed. To be sure, we have at Harvard no treasures comparable to those which have now found a permanent home at the Ashmolean. The oldest Harvard instruments, astronomical instruments of the Colonial period, are new as compared with some of those which Mr GUNTHER has described. Yet they are exceedingly precious, for they are witnesses of the first scientific endeavor made in the New World. They may be hardly more than two centuries old, but as time goes by, they become older and older. They need protection and care. They are at present scattered in various University buildings (e. g. many of them have been shut up in the cellars of the Music Building!) and are practically unknown. We hope that at some time, not very remote, it will be possible to collect them and exhibit them in simple but beautiful surroundings, that they may be preserved from further loss and injury, that they may be at once a memorial of past accomplishments and an inspiration towards new and greater ones, that they may be like magnets attracting similar old relics which are now lost in obscure places and in constant danger, that they may bring them all together in one beautiful place for their common safety and for the perpetual instruction, recreation and stimulation of generations to come.

G. S.

V. Miscellanea.

Introduction to the History and Philosophy of Science. — The work undertaken in the winter of 1920-21 and of which I gave a preliminary outline in *Isis*, IV, 23-31, 1921, is progressing well but very slowly. I have published brief accounts of my annual progress in the

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(1) About which see *Isis* VI, 449-453.

Carnegie Institution Year Books (1). Some of the data included in my latest report (for the period Sept. 1, 1922 to Aug. 31, 1923) may possibly interest a larger circle of readers and therefore, I hope to be pardoned for reprinting here a short extract.

« I have written 1,248 notes (ranging from a few hundred to many thousand words) dealing with the most prominent men of science and

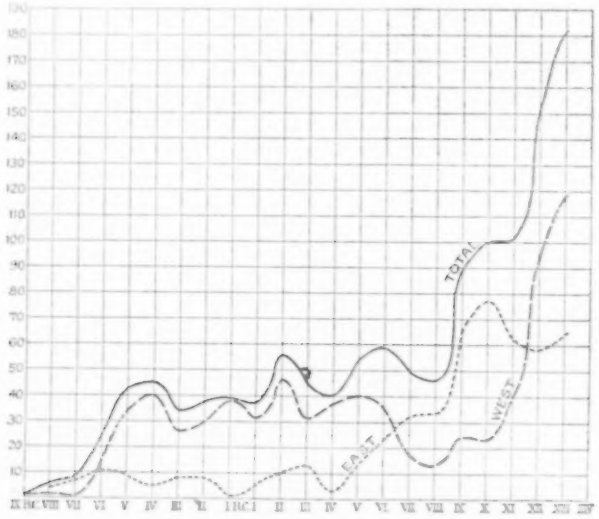

FIG. 1. — Progress of thought from ninth century B. C. to thirteenth century of our era. Of the three curves, one represents the total number of prominent scientists and scholars, others the partial totals relative respectively to East and West.

scholars and the most significant anonymous works down to the end of the thirteenth century (2). These 1,248 scientists may be divided into four main groups according to the languages which they used : 1. Greek, Syriac, Armenian, etc., corresponding to the ancient Greek world and to Eastern Christendom, 362; 2. Latin and European vernaculars, corresponding to Western Christendom, 373; 3. Hebrew, Aramaic, Arabic, Persian, corresponding to Israel and Islam, 324; 4. Sanskrit, Tibetan, Chinese, and Japanese, corresponding to India, Central Asia, and the Far East, 189.

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(1) No. 20 (1921), 426-7; No. 21 (1922), 366-7; No. 22 (1923), 357-7.

(2) I have not taken into account my notes on the fourteenth century because my study of that century is not yet finished. (August 31, 1923.)

« Some readers may be surprised that the numbers relative to the first three divisions are very close to one another (362, 373, 324) and that the one relative to the fourth, 189, is not so distant from the others as they might have thought. This will help them to realize that the traditions bequeathed by classical antiquity do not by any means form the whole of our intellectual heritage.

« The compass of my investigations is well illustrated by the two annexed diagrams. In both of them, the abscissas indicate successive

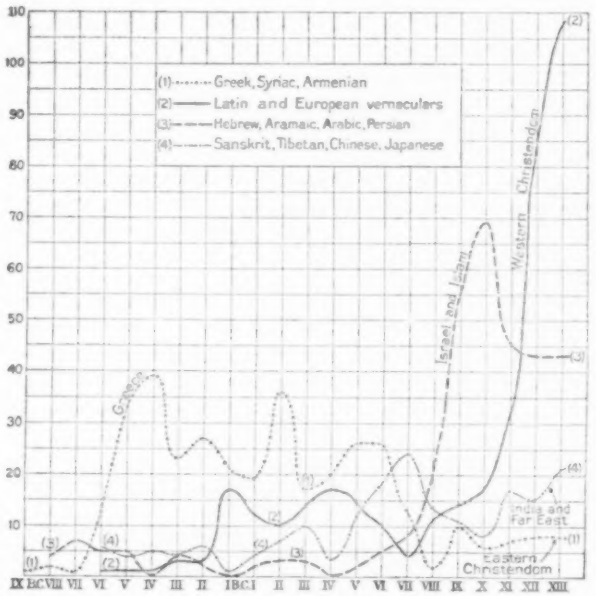
(1) Greek, Syriac, Armenian
(2) Latin and European vernaculars
(3) Hebrew, Aramaic, Arabic, Persian
(4) Sanskrit, Tibetan, Chinese, Japanese
Greece
Israel and Islam
Western Christendom
India and Far East
Eastern Christendom
IX VIII VII VI V IV III II I A.C. I II III IV V VI VII VIII IX X XI XII XIII

Fig. 2. — Relative progress of thought among the people speaking : (1) Greek, Syriac, Armenian ; (2) Latin and European vernaculars ; (3) Hebrew, Aramaic, Arabic, Persian ; (4) Sanskrit, Tibetan, Chinese, Japanese.

centuries, from the ninth before Christ to the thirtheenth of our era, while the ordinates indicate numbers of scientists. These diagrams represent the progress of thought, but only to a certain extent, because the number of great scientists and scholars flourishing at any period is but one indication among others of its intellectual activity. The importance of some of these scientists is immeasurably greater than that of others. If it were possible to give them definite weights or coefficients before adding them up, the parts of the curve relative to ancient Greece, for example would be much higher. Besides, the number of scientists tended naturally to increase throughout the ages,

irrespective of the progress of science, together with the area of the civilized world.

« The diagrams should be considered qualitatively rather than quantitatively. The curves aid one to visualize the amount of scientific activity accomplished at different periods by different provinces of mankind. For example, figure 1 gives one a good idea of the relative importance of East and West. One sees that down to the sixth century the West was preponderant, but that, on the contrary, from the seventh to the eleventh century the higher destiny of mankind was fulfilled mainly by Eastern peoples. Figure 2 enables one to appreciate the relative contributions of the four groups of peoples above mentioned. Thus, one notes that Greek influence was paramount down to the sixth century; then for one century, the Hindu and Chinese; and from the eighth to the eleventh century, and to an astounding extent, the Muslim. This makes it clear why knowledge of Arabic is as essential for understanding mediaeval thought as knowledge of Latin and Greek. Indeed, it is indispensable for a deeper study of the eighth to eleventh centuries. After the eleventh century, Western Christendom has been the main leader. »

The two diagrams which I was enabled to reproduce here by the courtesy of the Carnegie Institution, are, I believe, of special interest, in spite of the fact that they are only approximatively correct and do not even represent the present status of my own investigations. They explain more vividly than could be done in a great many words, why more attention should be paid to the study of oriental science if we would fully understand the transition between ancient and modern science. This justifies the efforts made by the editors of *Isis* to obtain more abundant information on oriental, and chiefly on Muslim science. Those subjects indeed are the largest « terrae incognitae » in our maps of the development of human progress. G. S.

Reviews

Joannes de Ketham Alemanus. — *Fasciculus Medicinae, 1491.* Facsimile of the first edition with an historical introduction and notes by KARL SUDHOFF translated and adapted by CHARLES SINGER. (*Monumenta Medica*, edited by HENRY E. SIGERIST, I). Large folio. R. LIER, Via Brera, 7, Milan 1924 (§ 10; Edition de Luxe, § 15, post free).

Before speaking of this very remarkable facsimile edition of one of the most famous medical books, it is well to state the general purpose of the collection which it so fittingly inaugurates. Says the publisher: « The object of this series is to make accessible to a wider circle of readers some of those monuments of medical science of which only a few copies now exist... It is our purpose to reproduce some of the most important works of all times and all countries, manuscripts, incunabula and later printed books. Books whose value largely depends on their illustrations will find a special consideration, and many works will be published in our series, which are important in the history of book-illustration and typography. Our intention however is by no means merely to publish a collection of fine books, but our choice will be controlled by the scientific interest of the books under consideration. Each volume will contain a study on the work reproduced and on its author and his influence. This introductory matter will be undertaken by a competent expert of the theme. The history of book-illustration will be explained by special plates. We have succeeded in securing the cooperation of several of the greatest authorities in this line, among them Prof. KARL SUDHOFF in Leipzig, Dr. CHARLES SINGER in London and Prof. GIOVANNI CARBONELLI in Rome. Each volume will be published simultaneously in an English and an Italian edition and either a German or French edition.

« A start is made with one of the most valuable books printed in the fifteenth century, the *Fasciculus Medicinæ* of JOHANNES DE KETHAM. This remarkable collection of medical tracts, after a century of circulation in manuscript, was first printed at Venice 1491, as a large folio-volume with superb plates. It will be edited in our series by Prof. KARL SUDHOFF who has studied the KETHAM-problem for a great many

years. The illustrations of this first edition are still derived from the conventional tradition of the Middle Ages. The Italian version printed two years later however betrays an entirely new spirit, the spirit of the Renaissance. There are more plates and we find among them the first anatomical representations not copied from a picture but drawn from nature. These are the first ones of their kind that were reproduced by printing. Dr. Charles Singer will edit this Italian translation of 1493, together with a study on the different editions and translations of the Ketham, on their effects and their later influence. The third volume of the Monumenta will present in facsimile the first ten printed papers on Syphilis of the years 1495-1498, and will show us how the disease was regarded in the literature within the few years which follow the date of its general recognition. This volume will be revised by the most competent authority on this subject, Prof. Sudhoff. The further volumes will deal above all with the anatomical works of the Renaissance, with the precursors of Andreas Vesalius. The « Isagogae breves. . in anatomiam humani corporis » of the Bolognese anatomist Jacopo Berengario da Carpi will follow soon. Vesalius himself will be represented by his Epitome. But also rare manuscripts such as the Codex Neapolitanus of Dioskorides and the London French Surgery of Roger with its lovely miniatures will rapidly follow. In connection with the Monumenta a volume of illustrations bearing on the history of medical science will be published, collected by Prof. Sudhoff and Dr. Sigerist. These will place in convenient and easily accessible form the most important but very scattered material from Ancient, Mediaeval and Renaissance times. We thus hope to help all lovers of the past of medicine to obtain a more direct access to the most important monuments of their study and thus to provide them with material for future investigations. »

The present volume contains a complete facsimile of the incunabulum with its six full-page woodcuts, representing respectively a circle of urinals, the vein-man, the zodiacal man, the pregnant woman, the wound-man, the disease-man. Sudhoff's introduction, as adapted by Singer « to the traditions of learned writing in our tongue », is a very complete study of the Fasciculus with special reference to its antecedents. As already said, this Fasciculus was not in any sense, when it appeared at Venice in 1491, a new publication. It was already well known in manuscript. Yet the enterprising printers, Giovanni and Gregorio dei Gregorii, who produced it, did something new - for never had so short a text been published with woodcut illustrations of such size and relative excellence. This was not their first venture as medical publishers, be it noted, for they had produced in 1489, the

Summa Conservationis of WILLIAM OF SALICETO and, in 1490, the *Compendia* of AVENZOAR and AVERROES.

The Fasciculus is the first work in which a *series* of didactic medical illustrations appear. But apart from the almanacs, two didactic medical diagrams had been printed before, to wit : a very crude phlebotomy figure in a Flemish surgical collection, Louvain 1481 (in Flemish) and a representation of the three cerebral ventricles in ALBERTUS MAGNUS, Philosophia naturalis, Brescia 1490.

Who was this JOHAN OF KETHAM ? His name appears in the explicit of the first edition, disappears entirely from the four Italian and Spanish editions which followed, and reappears in the title of the second Latin edition (1495), this time with the qualification « Alemanus », the Alemanian. Dr. SUDHOFF concludes that he should be identified with one JOHANN VON KIRCHHEIM (or KIRCHHAIM), who was professor at Vienna about 1460. This identification is plausible but far from certain, though I am not aware that any other has been suggested.

The largest part of the introduction is devoted to the manuscript antecedents of the six woodcuts of the *Fasciculus medicinæ* and is splendidly illustrated by means of thirteen full-sized facsimiles of manuscript diagrams (one of them (1) being coloured by hand). For the details of this iconographical study, I must refer the reader to the book itself.

This first volume of *Monumenta Medica* is very properly dedicated to the memory of GUSTAV KLEIN of Munich who had started many years ago a collection of the same kind (see *Isis*, I, 271-3) and had actually formed the project of including the Fasciculus in it. Were Dr. KLEIN alive he would certainly have admired and encouraged Dr SIGERIST's undertaking, a noble continuation of his own.

GEORGE SARTON.

E. E. Fournier d'Albe. — *The life of Sir* WILLIAM CROOKES, O. M., F. R. S. With a foreword by Sir OLIVER LODGE. xx+413 p., 4 portraits. London, FISHER UNWIN, 1923.

This life, pleasantly written, is a valuable contribution to our knowledge of the evolution of physical sciences during the second half of last century The personality of Sir WILLIAM CROOKES is unusually interesting in many ways. Like CAVENDISH, JOULE, HUGGINS and others, he was an amateur; he had no university education; his knowledge, however deep on certain points, was very heterogeneous; he never held a professorship; finally, he was a curious combination of inventor

(1) Eight of them in the edition de luxe.

and pure scientist, of business man and pure philosopher. The greatest part of his immense activity was devoted to industrial research; he had a large family to support and more than once he was in hopes of making a fortune, but, somehow, he always fell short of success, that is, of great financial success (1). His inventions were soon superseded by others, but his fame is solidly established upon a few discoveries in the field of pure science, discoveries, the importance of which can not b ut become more obvious as their applications multiply. Thus CROOKES failed to obtain the material rewards which he would have loved to offer to his wife and to bequeath to his children, but a greater and better reward was reserved for them.

To prove what I am saying, let me enumerate in the first place the many industrial investigations carried on by CROOKES throughout his life (it is in itself a very interesting catalogue of the problems which life suggested in his time),— then I will mention briefly the discoveries made by him within the same time.

Industrial research : photography; cattle plague (from 1865 to 1867, the *Rinderpest* destroyed more than 350,000 head of cattle in England; CROOKES's efforts to fight the plague resulted in popularizing the use of carbolic acid); gold-amagamation (CROOKES's soldium amalgam was soon superseded by potassium cyanide, a true solvent of gold); electric lighting (1878-89, he was finally beaten by EDISON and SWAN) (2); manufacture of aluminium (1888; it is amusing to read that CROOKES wrote in that year, that no company could pay its way unless it could sell a ton of aluminium per month, but he added that « a ton of aluminium would flood the market for many years »!), making of diamonds, etc.

Now to the scientific work. CROOKES's first published paper dealt with seleno-cyanides and was already, considering his tender age (he was not yet twenty) a masterly study. It is a pity that he did not think of the cyanides when he investigated gold-amalgamation fifteen years later, but it was his strange destiny to be regularly beaten in the field of invention by much smaller men.

1861. Spectroscopic discovery of thallium (rediscovered independently by the Belgian LAMY in 1862). CROOKES made a very accurate determination of its atomic weight in 1872.

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(1) He was very well off, and enjoyed at the end of his life an income of three to four thousand pounds a year. But that is nothing as compared with the earnings of many other chemists, much smaller men than himself.

(2) The house which CROOKES bought in 1880 (7, Kensington Park Gardens, London) and wherein he lived until his death and died, was the first house in England to be lighted by electricity (1880).

1875. Radiometer (explained by ARTHUR SCHUSTER in 1876).

1877. First description of cathode ray phenomena.

This last discovery was the result of CROOKES's success in producing, for the first time, a vacuum of the order of one millionth of an atmosphere. Gases rarified to such an extent were not gases any more. CROOKES spoke of having discovered a Fourth State of matter, as far removed from the state of gas, as a gas is from as liquid. We now realize that he was right, and more deeply so than he could dream of. What he had done, indeed, was to split matter into its smallest constituents, electrons. (1).

In 1881 CROOKES made a study of the viscosity of gases in high vacuum, confirming MAXWELL's law that the viscosity is independent from the pressure.

His genius appears as obviously in some of his clear anticipations as in his actual discoveries, though his fame, of course, rests upon the latter, and only subsidiarily upon the former. Thus an old notebook of his shows that he had anticipated to some extent, in 1854, the law of KIRCHHOFF and BUNSEN, according to which a metallic vapour absorbs the rays of the wave-length of those which it emits (2). His theories on radiant matter implied bold anticipations of many facts which have kept the scientific world in an almost continual state of excitement for the last thirty years. The same can be said of his views on the evolution of chemical elements. His address as President of the Institution of Electrical Engineers in Jan. 1891, contains a clear forecast of our isotopes. A paper published in the *Fortnightly Review*, Feb. 1892, shows that he had already grasped then, before MARCONI, the possibility and the fundamental problems of wireless telegraphy.

I repeat that anticipations are not discoveries, and I speak of the former only as additional evidences of the complexity of his genius. If one adds to the many industrial and scientific investigations which I have mentioned, a considerable amount of literary work (he wrote many books and articles and was, since 1859, editor of a scientific weekly *The Chemical News*), one realizes that he must have been a man of im-

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(1) By the way, the godfather of the electrons, JOHNSTONE STONEY of Dublin and CROOKES were friends since 1879. The notion of electron was introduced by STONEY in 1881.

(2) Spectroscopy was one of his first fields of study and remained a favorite one until the end. When WILLAM RAMSAY and Lord RAYLEIGH discovered argon independently in 1895, it was CROOKES who, comparing the spectra of samples received from each discoverer, proved their identity. In the following year, he was able to prove the identity of another rare gas, then just extracted by RAMSAY from cleveite, with the helium discovered by NORMAN LOCKYER in the sun.

mense vitality and almost uncanny energy. We are not surprised then to hear that he came from a very powerful stock. His father died of sheer old age at 92, and there were many centenarians among his paternal ancestors. Thus his family memory could easily reach back to the time of the Great Plague. His father, who was a succesful London tailor, begot twenty-one children, five by one wife and sixteen by another, WILLIAM being the eldest of the second batch (1). WILLIAM himself had ten children, of which seven survived childhood, one of these dying at thirteen.

There is still another side of CROOKES's activity which I have kept for the end, — his attempt to investigate « spiritualism » or the « psychic force » in a scientific manner (2). In spite of the fact that he failed to convince the scientific world of the reality of psychical phenomena, his failure was not a dishonorable one. Four years of his life (1871 to 1874) were largely devoted to the study of that mysterious subject. The author of this biography has given sufficient space to this (p. 176-239), without any disproportion. His account strikes me as very good and wise; he was exceedingly well prepared to write it (3). Why did CROOKES abandon his investigations in 1874? My impression is that he abandoned them chiefly — (as he would have done for any other investigations) — because he felt that he had done all that he could do. Is not that a plausible and sufficient reason? (4). It is corroborated by the fact that, though many have tried, nobody has gone further than CROOKES. Says FOURNIER D'ALBE « CROOKES covered the whole ground. He marked out the boundaries of the physical basis of spiritualism, and no subsequent investigator has been able to extend them. Indeed, no subsequent investigator has been able quite to cover the same ground. Twenty-five years later, when President of the British Association, CROOKES said : « I have nothing to retract. I adhere to my already published statements. I only regret a certain crudity in those early expositions which, no doubt justly, militated against their acceptance by the scientific world ». That world has since become more tol-

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(1) He was born in London on 1832.06.17 and died there on 1919.04.04.

(2) The reader may not realize how new modern spiritualism is. It was founded by the three Fox sisters in America in 1848. CROOKES used one of them, KATE FOX, as a medium.

(3) See his previous works : New light on immortality. London, LONGMANS, 1908; Translation of Dr. von SCHRENCK-NOTZING's Materialisations-Phaenomene, 1920; The GOLLIGHER Circle. London, WATKINS, 1921.

(4) In his last utterance on the subject, his Presidential Address to the B.A.A.S., Bristol, 1898, he suggested that telepathy offered the best means of attacking the problem.

erant and more elastic. Its outlook is wider, its inquiry more profound. It admits much in psychology and psycho-physiology that used to be doubtful and obscure, but it does not admit spiritualism : it does not even admit its « physical phenomena ». Spiritualism as a religion may legitimately be studied in a section of anthropology, but spiritualism as a science does not exist. To be a spiritualist, the scientist must surrender his wishes, his methods, his views into the hands of his « spirit friends » on the « other side ». If he does that he may achieve a certain peace of mind, but his scientific work will be at an end. His surrender may soothe him on his dead-bed, but so may almost any religion when embraced with a fervent faith. And we may expect that the world's work will best be done by those who follow the light of reason to the utmost visible horizon, content in the belief that the divine spark within us is but the promise of a greater glory as yet unrevealed. »

This conclusion may irritate some readers; it will please those who like myself consider spiritualism in the spirit of pure agnosticism, without any bias in any direction, and who are ever ready to believe everything with experimental proof and nothing without. CROOKES's personality, say Sir OLIVIER LODGE, « was not specially impressive. » It may amuse the philosopher to reflect that the fame of this unimpressive man will ultimately depend upon the evolution of psychical research. If this proves to be a will-o'-the-wisp, CROOKES will be thought of later as a great physico-chemist, the discoverer of thallium, of the radiometer, the first explorer of highly rarified matter, a great man of science to be sure, but not greater than a good many of his contemporaries. But if it is not a will o'-the-wisp (and who can tell?), CROOKES will eventually emerge as one of the greatest pioneers of the race, as a hero among heroes, bolder than the rest of them... Historians are supposed to know the past, not the future. But their longer and richer experience makes it easier for them (if they have any imagination) to anticipate with aequanimity the most extraordinary developments (1).

G. SARTON.

T. Eric Peet. — *The Rhind mathematical papyrus*, British Museum 10057 and 10058. Introduction, transcription, translation and commentary. Folio 136 p., 24 plates. The University Press of Liverpool. HODDER and STOUGHTON, London, 1923. [63 s.]

" Enter PEET, exit EISENLOHR ". EISENLOHR's translation and explanation of the RHIND papyrus appeared at Leipzig in 1877 and had

(1) The author has published a correction to this work in *Nature*, vol. 113, 617, 1924.

remained ever since the main source for the study of Egyptian mathematics. An admirable facsimile edition of the papyrus had been published by the British Museum in 1898, it is true, but this facsimile, however precious, was of no use to historians of mathematics ignorant of the Egyptian language. Thus these historians were obliged to depend almost exclusively upon EISENLOHR's interpretation. They had done so at first with great confidence, but the progress of Egyptology introduced gradually various corrections and their confidence was slowly undermined. Among the works which had obliged them to correct their views, I will only quote F. LL. GRIFFITH's papers in the *Proc. of the Society of Biblical Archaeology*, vol. XIII, XIV, XV, XVI, 1891 sq. and his edition of the Kahun and Gurob papyri, London 1898; F. HULTSCH, " Die Elemente der ägyptischen Teilungsrechnung ", *Abhdl. der Sächsischen Ges. d. Wiss., phil. Kl.*, vol. XVII, 1895; KURT SETHE, " Von Zahlen und Zahlworten bei den alten Aegyptern ", Strassburg, 1916 (*Isis*, IV, 179). Within the last years EISENLOHR's admirable edition (for admirable it was and we would be egregiously unfair and ungrateful if we failed to recognize it) was accepted *faute de mieux* and the scholars, who knew that the professor of Egyptology in the University of Liverpool was preparing a new edition and translation, were anxiously awaiting its publication. After twelve years, their patience is finally rewarded.

In professor PEET's translation, the papyrus begins as follows : " Rules for enquiring into nature, and for knowing all that exists, [every] mystery, ... every secret. Behold this roll was written in Year 33, month 4 of the inundation season, ... [under the majesty of the King of Upper] and Lower Egypt AAUSERRÊ', endowed with life, in the likeness of a writing of antiquity made in the time of the King of Upper and Lower Egypt NEMARÊ'. It was the scribe AḤMÔSE who wrote this copy. " The Hyksos King AAUSERRÊ' APOPHIS must have ruled at sometime between 1788 and 1580. Says the learned editor : " There is no reason to doubt the scribe's own statement that it was a copy of an older document written in the time of King NEMARÊ', AMENEMMES III of the XIIth Dynasty, who was on the throne from about 1849 to 1801 B. C. It is thus the latest of our hieratic mathematical papyri, the Moscow Papyrus and the Kahun and Berlin fragments all being definitely XIIth Dynasty documents : its prototype clearly belonged to the same milieu as these. " The Moscow papyrus here mentioned is still unknown to us, beyond the short account of it given by TURAIEV in *Ancient Egypt*, 1917, p. 100-102. That account was rather tantalizing for according to it, the Moscow papyrus contains the correct determination of the volume of a truncated pyramid on a square base. However, Professor PEET, who has been

able to study photographs of the whole papyrus (to be eventually published by Professor STROUWÉ), says that it contains nothing else which will greatly modify our conception of Egyptian mathematics.

I suppose that the reader of this review is already familiar with the RHIND papyrus and I may thus limit my account of it to a few remarks borrowed from the editor's excellent introduction. In the first place, it is perhaps not generally known that some fragments of this very papyrus are kept in the museum of the New York Historical Society, together with the EDWIN SMITH medical papyrus of which J. H. BREASTED is preparing an edition (1). It has been generally assumed that the two British Museum papyri 10057 and 10058 were originally one. The New York fragments place this beyond doubt for they help to fill the gap between the two London sheets. The introduction contains a brief analysis of all the documents available for the study of Egyptian mathematics, not only the early documents but also the later ones which prove how long Egyptian traditions lingered in spite of the immense superiority of Greek knowledge. To the list of later documents might be added the Michigan mathematical papyrus No. 621, partly published by L. C. KARPINSKI in *Isis*, t. V, 20-25, with facsimile.

Egyptologists are more and more inclined to consider the Old Kingdom as the Golden Age of Egyptian knowledge and wisdom. There is no definite evidence for ascribing so great an antiquity to mathematics, however. " All we know is that by the beginning of the First Dynasty the system of notation was complete up to the sign for 1,000,000. In the IVth Dynasty we find in the tomb of Methen that the land measures of the RHIND Papyrus are already in full development in a form which involves correct determination of the area of the rectangle, but not of necessity of the triangle or circle. There appears to be no early evidence with regard to measures of capacity, though one may almost take it for granted that with the measurement of the field on which the corn was grown went that of the containers in which it was stored and sold. That measurement by weighing was practised can hardly be denied in view of various objects of Old Kingdom date which can scarcely be anything but weights, as for example the stone weight of Khufu, formerly in the HILTON PRICE collection, though the attempts to establish a standard from these objects have been far from satisfactory. From these feeble indications we pass straight to the fully developed mathematical system of the

(1) Apropos of this see *Isis* V, 447-9.

XIIth Dynasty, the early stages in the building up of which are entirely concealed from us." The greatest part of the introduction is devoted to a very complete and very lucid analysis of Egyptian mathematics, a briefer account of weights and measures (1), a comparison of Egyptian with Sumerian mathematics and a discussion of Greek knowledge on the subject. The discussion of DEMOCRITUS' reference to the *harpedonaptai* is particularly interesting. I quote it in full but without the foot-notes (p. 32). "Who were these *harpedonaptai*? More than one historian of mathematics has supposed that they were landmeasurers. The literal meaning of the word is "rope-stretchers", and it is suggested that they were acquainted with the fact that a triangle whose sides were 3, 4 and 5 contained a right-angle, and that they constructed right angles accordingly, as did the Chinese and the Indians. For this last statement I can find no foundation whatsoever; nothing in Egyptian mathematics suggests that the Egyptians were acquainted even with special cases of PYTHAGORAS' theorem concerning the squares on the sides of a right-angled triangle. That the *harpedonaptai* were land-measurers on the other hand is most probable, indeed we can even see such persons at work in the pictures on the walls of Egyptian tombs. In the tomb of KHAEMHET at Thebes we see a number of men equipped with ropes and writing material measuring a field, and there is a similar scene in the tomb of MENENA. In either case the persons engaged in the work might most suitably be described as "rope-stretchers"; the very unit by which fields were measured was a "reel of rope" of 100 cubits in length. This process of land-measuring with a rope, the Egyptian name for which is not known, has been confused by historians of mathematics with the ceremony of "the stretching of the cord". This was one of the initial ceremonies at the foundation of a temple. The King or whoever represented him took a sighting of the pole-star through a cleft stick, another person standing north of him with a plumb-bob attached to a wooden arm. Each then drove a stake into the ground in front of him, and a cord stretched between the two gave a true north and south line and enabled the four corners of the temple to be fixed. Here the stretching of the cord is used not necessarily in measurement, but in the fixing of the orientation. Possibly it was something of this kind which DEMOCRITUS had in mind when he spoke of the skill of the *harpedonaptai*."

The editor insists perhaps a little too much upon the practical and empirical nature of Egyptian mathematics. Of course, these early papyri are very practical and on a low intellectual level as compared

(1) Egyptian weights and measures have been admirably explained and discussed by F. LL. GRIFFITH in the papers above-mentioned, 1891 sq.

with the Greek writings which appeared at least a thousand years later. But is the writing of a treatise not already an immense step forward in the right direction? The very arrangement of the treatise implies that a substantial amount of rationalisation and theorisation had already been accomplished. It is unfair to conclude from these early documents (1), that the Egyptian mind was unscientific, though I admit that such conclusion may be fairly drawn from the comparative study of *later* papyri : the Egyptian genius was precocious but its development was arrested relatively soon and at a relatively low stage. Was this stagnation due to inherent vices, to a constitutional inferiority, or to external circumstances?

The faithfulness of this new translation can not be judged except by Egyptologists. All that I can say is that the learned professor of Liverpool has done his work with admirable thoroughness (2) and that he has been able to avail himself not simply of EISENLOHR's translation but of the many special studies which have since been published. Both his translation and his commentary are very clear. From now on it will be obviously impossible to touch the subject of Egyptian mathematics without a serious study of his work. The introduction itself is very valuable and every student of the history of mathematics ought to read it and to assimilate its contents. The material presentation of the results of Professor PEET's investigations does credit to the University of Liverpool.

GEORGE SARTON.

Robin, Léon. — *La pensée grecque et les origines de l'esprit scientifique*. XXIV + 480 p. in-8°, 1 carte hors texte ; vol. XIII de l'*Evolution de l'Humanité*, dirigée par HENRI BERR, La Renaissance du Livre, Paris, 1924. [15 Fr]

Dans l'introduction de sa copieuse étude sur *La pensée grecque et les origines de la pensée scientifique*, ROBIN oppose l'histoire de la science à l'histoire de la philosophie, celle-ci, dit-il, « satisfaisant les besoins les plus généraux et les plus profonds de la pensée », tandis que l'histoire de la science n'est que « ce qu'il y a de mort dans l'effort du passé vers la vérité, ou bien l'effort oublié quand le but est atteint » (pp. IV et V). Nous ne pouvons, dans cette *Revue*, consacrée à l'histoire de la science et de la civilisation, c'est-à-dire à l'histoire de la pensée tout court, souscrire à une telle façon de voir les choses. Au reste, HENRI BERR, dans l'avant-propos qu'il consacre à ce volume de la collection qu'il dirige, s'élève lui-même contre cette conception, et je ne saurais

(1) See a corroboration of this in my remarks on the EDWIN SMITH papyrus in *Isis* V, 448.

(2) To have an idea of the editor's thoroughness, read his discussion of problems 41 to 47 dealing with volumes and cubic contents, p. 80-88.

mieux faire que reproduire ici les quelques lignes dans lesquelles il oppose son point de vue à celui de son collaborateur : « L'histoire de la science n'est pas une chose morte, une curiosité d'érudit... La science amasse peu à peu des matériaux positifs qui permettent de soumettre à l'épreuve les vues *a priori* de l'esprit : elle est bien un *organum*, l'instrument de résolution des problèmes philosophiques. L'histoire de la science est nécessaire à l'histoire de la pensée, parce que l'instrument du progrès de la pensée, c'est la science. » (p. XVI.)

Étant donnée la façon dont le problème est envisagé par ROBIN, on ne trouvera dans son livre que l'histoire de la pensée philosophique, — dans la mesure tout au moins où la science peut en être détachée — depuis ses premières expressions littéraires jusqu'à la fermeture de l'École d'Athènes en 529, le développement de la pensée chrétienne en étant exclu, ce qui est fort légitime, puisqu'il dérive d'autres tendances et s'oppose à la philosophie proprement hellénique.

Après deux courts chapitres sur les origines, l'auteur étudie sous le titre général : *Science et philosophie* (114 p.), le premier moment de la période de la formation de la pensée grecque, qui se caractérise par l'union intime de la science et de la philosophie, et que l'on désigne habituellement sous le nom de période présocratique, bien qu'elle se prolonge dans le temps de SOCRATE et même après lui (l'École de Milet, le Pythagorisme, HERACLITE, l'École d'Elée, EMPEDOCLE, l'École d'Abdère, ANAXAGORE). Suit un livre (225 p.) intitulé *La culture humaine*, qui correspond à la fois au second moment de la période de formation (les Sophistes, SOCRATE et les Écoles d'Elis et d'Erétrie, l'École de Mégare, l'École cynique, l'École de Cyrène), et à la période de maturité de la pensée grecque (PLATON, ARISTOTE et leurs disciples immédiats). Enfin, la dernière période, qui s'étend sur six siècles environ, dans laquelle s'accomplit d'une part un énorme travail d'exégèse sur l'œuvre de PLATON et d'ARISTOTE, tandis que d'autre part s'affirme de plus en plus la tendance à rapprocher et à fondre les philosophies antagonistes, et qu'aussi apparaît la tentative de restauration de PLOTIN, est étudiée en deux parties (90 p.).

Si je me place un moment au point de vue particulier de ROBIN, je me plais à reconnaître que son livre, qui devrait alors avoir pour titre : *La philosophie grecque*, répond à l'objet qu'il s'est proposé ; les hommes et les idées (le texte des penseurs formant la substance même de l'exposé) revivent parfaitement dans leur cadre social ; pas de vain appareil d'érudition, mais de nombreux renvois à une très solide bibliographie.

Malheureusement, de tous les volumes qui doivent constituer cette collection de l'histoire de l'*Évolution de l'humanité*, celui-ci était le seul où il pût être question de l'histoire de la science hellène, qui se trou-

vera ainsi passée sous silence : Euclide, Archimède. Apollonius de Perge, Héron d'Alexandrie, Pappus, Diophante, Aristarque de Samos, Hipparque de Nicée, Théon de Smyrne, Claude Ptolémée, disposent pour eux tous d'une page; Herophile (dont le nom a été oublié dans l'index), Érasistrate, Athénée d'Attalie, Archigène, Arétée, Thémison, Celse, sont cités en quelques lignes. Je me refuse à concevoir qu'ils soient si peu de chose dans l'histoire de la pensée grecque, ni dans les origines de l'esprit scientifique.

L. Guinet.

C. H. Becker. — *Islamstudien. Vom Werden und Wesen der islamischen Welt*. Leipzig 1924, XII und 534 S. Quelle & Meyer. [Geb. 14 RMk, geb. 20 RMk.]

Ist dieses Werk auch keine neue Darstellung, sondern eine Zusammenfassung der von 1904 bis in die letzte Zeit erschienenen Studien des Verfassers, so gebietet doch ebenso der Gegenstand wie die hervorragende Bedeutung dieser gesammelten Studien eine Besprechung des Bandes in dieser Zeitschrift. In engeren Kreisen, bei Orientalisten und Historikern, längst gebührend geschätzt, verdienen die Arbeiten des Begründers der Zeitschrift *Der Islam*, des jetzigen Staatssekretärs im Preussischen Ministerium für Wissenschaft, Kunst und Volksbildung von jedem an der Kulturgeschichte des vorderen Orients Interessierten wegen ihrer klassischen Form und wegen ihrer tiefdringenden Fragestellungen studiert zu werden. So verschiedenartig auch immer der Anlass und Inhalt der Arbeiten ist, sie spiegeln alle den feinsinnigen Interpreten islamischen Lebens wieder, sie schliessen sich als « organische Teile eines werdenden Ganzen » zu einer inneren Einheit zusammen.

Die Abhandlung, mit der C. H. Becker vor 14 Jahren den ersten Band seiner dem Islam gewidmeten Zeitschrift eröffnete, steht auch an der Spitze dieses Bandes. Der Islam wird als Problem aufgefasst : Wie kommt die Einheitszivilisation des Islam zustande, und welche Rolle spielt das religiöse Moment in diesem Entwicklungsprozess? Die Antwort lautet : nicht die islamische Religion hat die einheitliche Zivilisation erzeugt, sondern die aus ganz anderen Gründen entstandene einheitliche Zivilisation des Chalifenreiches ist die Voraussetzung für die Ausdehnung und das bis in die Gegenwart hinein siegreiche Vordringen der islamischen Religion. Dass die Vorstellung von der « arabischen Kultur » und ihrem Siegeslauf nichts ist als ein Märchen, hat vor und nach C. H. Becker schon mancher Orientalist versichert, und doch freut man sich wieder der markanten Kennzeichnung (S 16), die die Kritik von Spenglers arabischer Kultur vorwegnimmt. Der *Hellenismus* ist die Wurzel jener Einheitszivilisation.

Die Einheitlichkeit der westasiatischen Kultur seit der Diadochenzeit ist mit Händen zu greifen. Die Grenzen zwischen Ost und West, Persien und Byzanz, werden immer fliessender. Den vollendetsten Ausdruck der Mischung bietet die aramäische Christenheit. Die islamische Kultur und Zivilisation ist die Weiterbildung der aramäischen im Sinne einer fortschreitenden Asiatisierung. Die wirksame Kraft geht aber nicht so sehr von der Religion aus, als von der durch sie mit bedingten politischen Einheit Vorderasiens. Von der Verlegung der Residenz nach Bagdad an ist das Kalifenreich nichts anderes mehr als eine Fortsetzung des Chosroenreiches auf breiterer politischer Grundlage.

Der Vortrag, den C. H. Becker auf dem Deutschen Orientalistentag zu Leipzig 1921 gehalten hat, schliesst sich sinngemäss an den einleitenden Aufsatz an. Der Islam wird « im Rahmen einer allgemeinen Kulturgeschichte » vom eigentlichen Asien abgetrennt und der europäisch-amerikanischen Welt angegliedert. Man braucht nur an die Kulturlage im Mittelalter zu denken, um diese Entscheidung als selbstverständlich zu empfinden. Unter die einleitenden Aufsätze sind auch die während des Kriegs erschienenen Arbeiten *Der Islam als Weltanschauung* und *Islam und Wirtschaft* eingereiht.

Acht Arbeiten *Zur Geschichte und Kulturgeschichte der Chalifenzeit* bilden die nächste Gruppe. Vor allen ragt darunter die erste umfangreiche Abhandlung hervor, die ursprünglich englisch unter dem Titel *The Expansion of the Saracens* 1912 im Verlag der Cambridge University Press erschienen ist. Die übrigen betreffen Steuer- und Wirtschaftsprobleme, die weniger im Interessenkreis dieser Zeitschrift liegen; der letzte Beitrag ist kunstgeschichtlicher Natur, indem er drei ausführliche Studien, zur Mschattâ-Façade, zu Quṣair ʿAmrâ und zum Amida-Werk vereinigt. Ihre Lektüre wirkt immer wieder fesselnd bis zum letzten Satz, zumal auch heute noch der Kampf der Meinungen scharf hin- und herwogt.

Sieben Arbeiten über den *Islam als Religion* beschliessen den ersten Band. Unter ihnen sei besonders die in der Sammlung Religionsgeschichtlicher Volksbücher erschienene Abhandlung *Christentum und Islam* hervorgehoben. Ob sich die weitgehende Parallelisierung des islamischem Gottesdienstes mit der Messe wird aufrecht halten lassen, scheint mir fraglich. Ein Kabinetsstück Becker'scher Belesenheit und Darstellung ist der Aufsatz *Ubi sunt qui ante nos?*, der in der Festschrift zu E. Kuhns 70ten Geburtstage zuerst erschienen ist.

Die Frage liegt nahe, welchen Gewinn das Studium der hier vereinigten Abhandlungen für diejenigen haben mag, die sich die Geschichte der Wissenschaften im Islam als Arbeitsfeld gewählt haben oder wählen wollen. Man könnte zweifelnd fragen: was haben Koranexe-

gese und Hadith mit der Astronomie und Mathematik, was haben Kanzel und Minaret mit der Medizin zu schaffen? Sind diese Wissenschaften nicht ganz unabhängig von der religiösen Umwelt, entwickeln sie sich nicht nach eigenen Gesetzen jenseits aller Verschiedenheit von Glaubens- uud Völkergrenzen? Man kann dieses Eigenleben zugestehen und anerkennen, dass die innere Bewegung einer Wissenschaft nichts mit der Umwelt zu tun hat — und doch, ein lebensvolles Bild der islamischen Wissenschaft, ein Verständnis ihrer Lebensbedingungen, ihres Blühens wie ihres Verfalls ist ebensowenig möglich ohne tieferes Eindringen in das geistige Gesamtleben des Orients, wie ein GALILEI und KEPLER, ein BOYLE und LEIBNIZ verstanden werden kann ohne die Kenntnis der zeitgenössischen Gesamtkultur. Soll Wissenschaftsgeschichte kein blasser Schatten sein, so muss sie Blut und Leben aus der Umwelt aufnehmen, aus der die grossen Denker und Forscher selbst ihre geistige Nahrung gezogen haben. Wo wir uns auf eigenem Boden bewegen, empfinden wir das Bedürfnis vielleicht weniger stark; je weiter zurück und je weiter von unserer eigenen Kultur entfernt der Mann und sein Werk aber liegt, desto dringender ist die Aufgabe der lebensvollen Ergänzung. Hier also, für den Islam, ist in C. H. BECKERS Studien ein unschätzbares Hilfsmittel gegeben, sich in die fremde und im Grunde doch so verwandte — vor allem mit dem mittelalterlichen Geistesleben verwandte — Kultur des östlichen Hellenismus einzuarbeiten. In diesem Sinne sei mit Nachdruck auf die Sammlnng hingewiesen.

(*Heidelberg*). JULIUS RUSKA.

Maurice Gaudefroy-Demombynes, Professeur à l'École des Langues Orientales vivantes. — *La Syrie à l'époque des Mamelouks d'après les Auteurs Arabes*. Description géographique, économique et administrative précédée d'une introduction sur l'organisation gouvernementale. (Haut-Commissariat de la République Française en Syrie et au Liban; Bibliothèque Archéologique et Historique. T. III), CXX+288 p. GEUTHNER, Paris, 1923.

The promise of this title is fully kept in the book which may be unreservedly recommended to all interested in the Syria of the XIIIth and XIVth centuries. And its scope is even wider. For besides a bibliographical preface of 18 pages, there is a long and most valuable introduction of 101 pages on the political and administrative organization of the Mamluk state. It is true that the ground was broken for this study in scattered notes by QUATREMÈRE, HARTMANN, VAN BERCHEM and others but this, in spite of the modesty with which Professor GAUDEFROY-DEMOMBYNES speaks of his work, is the first connected treatment of a turning point in the development of the Muslim state.

In its ligl t the organization of the Ottoman state with its sultanate, its caliphate and its influence of the professional learned class of theologians and canonists, headed by the shaykh al-Islâm — the permanent staff which carried on the tradition of Islam — is indefinitely clarified. Guided by it we can « place » the recent events at Angora in a way that was not before possible. This part of the book, therefore, may be recommended to all students of constitutional law and to all publicists who are willing to take the trouble to read their way into it. It will offer to them no real difficulties as to technical language.

The 264 pages of geographical and economic description of Syria in the body of the book are of a more specialistic importance. They complete and correct with a wealth of detail LE STRANGE's books, especially that on « Palestine under the Moslems. » To those working on the ground and trying to understand the environment in which they work, these will form an indispensable hand-book; that ground may be put shortly as the territories connected with Damascus, Aleppo, Hama, Tripoli, Safad and al-Karak. There are appendices on the posts, by riders and by pigeons, established between Egypt and Syria by the Mamluk sultans, on the transport of snow to Egypt, on the system of signalling by fire and on the « burnt lands » instituted as a protection to the frontiers against Hulagu and his hordes. To do this they employed, according to reputable Arabic historians, SAMSON's method with the foxes' tails — a very curious bit of folklore survival.

There is an excellent index of place-names and another of names of persons. But even more generally useful will be a full index of the Arabic, Persian and Turkish words used and explained; this forms a really valuable addition to all lexicons and will have to be incorporated in them.

In a book containing so great a mass of details it will always be possible to find points to criticise, rightly or wrongly. I bring up here only one as it illustrates a general danger. On p. 57, in note 2, Professor GAUDEFROY-DEMOMBYNES is surprised that a fourteenth century Arabic authority finds it quite natural that a Christian priest should be allowed by a Muslim court to constitute a garden as a *waqf*, or estate in mortmain, for the benefit of a Christian church, and he cites JUYNBOLL's « Handbuch », p. 279, as authority against that possibility. In this, JUYNBOLL's brevity of treatment has misled him. If JUYNBOLL had gone on to translate one more word, *lit-ta'abbud*, he would have made it plain that such a *waqf* was forbidden in Moslem canon-law only if it were for specifically religious purposes and not, as evidently in this case, for the general advantage.

See Bayjûri's Corpus of Shâfi'ite canon-law, vol. II, p. 46 of ed. Cairo 1307 and SACHAU's « Muhammedanisches Recht » p. 612. In the case of so complicated a subject as canon-law it is never safe to stop short of the original authorities.

(*Hartford, Conn.*) D. B. MACDONALD.

P. J. André, capitaine d'infanterie coloniale (**Pierre Redan**). — *L'Islam et les Races :* Tome premier. Les origines, le Tronc et la Greffe ; Tome second. Les Rameaux (mouvements régionaux et sectes). XVI+270+326 p. Librairie orientaliste PAUL GEUTHNER, Paris, 1922.

The problem before France at the present time in relation to Islam is whether its interests should incline it towards the new Turkey of KEMAL Pasha and Angora or towards the non-Turkish, mostly Arabic, Muslim world. Every book on the Muslim question which has appeared in France for some years past and which was not scientific in the strictest sense, and some even which professed to be scientific, has taken up the one side or the other. This book is a frank plea on the Arabic side and its greatest interest for its readers who are not French is that it shows how a French administrator of wide experience — Kurdistan, Cilicia, the Berbers, Morocco — looks at the present-day Muslim world with all its complications, implications and problems, racial, social, economic, religious. It appeared in 1922 and it may quite fairly be claimed for it that the more recent developments of the Angora policy have justified its position. The action of Angora on the sultanate and the caliphate, putting the state on a purely secular basis, has thrown the new Turkey completely out of accord with the rest of the Muslim world and has made it plain that if the different Muslim peoples ever again find a champion of Islam among themselves and a centre for their religious and social lives and hopes, such as was the old-time Ottoman Sultan-Caliph in Constantinople, it will not be in this new Turkey. And those who wish to be on the side of any hypothetical united Islam must look elsewhere.

But M. ANDRÉ has aimed at much more than a plea for a policy. His desire has been to write such a history of political Islam from its beginnings down to the problems of the present day as may form a manual of all Muslim questions for French administrators, teachers and diplomatists — for all, in fact, except specialists in Islam. Always conscious of the very real Turkish-Arabic antagonism, which he sees truly as the clash between disciplined loyalty and individualism, he has constructed his book round the absorbing into the Muslim state of the different races which form the Muslim world and the re-actions upon Islam of these races, and their characteristics. Thus his first

volume is a broad history of political Islam from the origins, through the development, expansion and decline of the so-called Arab state to the coming and predominance of the Mongol races and the final hegemony of Islam by the Ottoman Turks seated in Constantinople. He brings his history down to the Turkish Assembly at Angora in 1920. If his book had been delayed for another year he would have had to record the vanishing of that hegemony through the secularist policy of KEMAL Pasha. His second volume covers in a sense, the same ground but is devoted to a detailed consideration of the different schisms and sects, religious and political, and to the local movements and developments which have produced separate states. It thus gives a view, and to a great extent an explanation, of the very many different local situations scattered over the world of Islam of our own time. Here M. ANDRÉ is at home and his work will be found of distinct value; it is clear and more trustworthy than most such broad views. The same unfortunately cannot be said of the first volume. It called for a scholar and a specialist in Islam and M. ANDRÉ would not, I think, claim to be either. He had to write it; for he had to find a background and explanation for the present-day phenomena which confront us. In some points he has strained the difference between the Arabic and the Turkish attitudes. No non-Muslim was ever a full citizen of an Arab state and the self-government of the non-Muslim communities within the Muslim states goes back to the earliest times of Islam and, in fact, to the Byzantine Empire itself.

(*Hartford, Conn.*) D. B. MACDONALD.

Rignano, Eugenio. — *La mémoire biologique*. Essais d'une conception philosophique nouvelle de la vie, 248 p., in-16. ERNEST FLAMMARION. Paris, 1923. [7 fr. 50]

RIGNANO a réuni dans ce volume dont une édition italienne a paru un an avant celle-ci (ZANICHELLI, Bologne) une série d'essais qui constituent une tentative nouvelle, très différente de celle de HERING (1870), et de SEMON (1904), de mettre la mémoire organique à la base de la biologie, sans pour cela tout réduire à la mémoire. A vrai dire, ce n'est pas d'aujourd'hui que date cette conception chez RIGNANO. Les premiers chapitres de la *Mémoire biologique* reprennent et résument son travail *Sur la transmissibilité des caractères acquis et l'hypothèse d'une centro-épigenèse* (1906), et dans sa *Psychologie du raisonnement* (*Isis*, IV, 90-93), la propriété mnémonique de la matière vivante joue un rôle de premier plan.

L'examen critique des doctrines transformistes, des controverses entre préformistes et épigénistes amène RIGNANO à la conclusion que la base de la vie réside dans des propriétés énergétiques particulières de

l'énergie nerveuse qui devient ainsi une véritable forme de l'énergie, obéissant aux lois particulières de l'énergétique, mais différant cependant par quelques propriétés « de toutes les autres formes d'énergie, de la même façon que celles-ci diffèrent entre elles » (p. 97). La principale de ces propriétés est celle-ci : chaque état physiologique laisse derrière lui, en un point déterminé de l'organisme, une accumulation spécifique d'énergie nerveuse, chaque accumulation étant apte à donner comme décharge uniquement cette même spécifité du courant nerveux de charge par laquelle cette accumulation aurait été déposée (p. 143).

A la lumière de ces hypothèses, d'un examen critique de la théorie de Semon, de l'examen d'objections faites à sa théorie par Botazzi, de confirmations apportées par Francis Darwin, Rignano explique le développement individuel des organismes, et le fait que l'ontogenèse est une récapitulation succinte de la phylogenèse, et la transmission des caractères acquis. Sa conception est ensuite appliquée aux instincts, aux tendances affectives, au fonctionnement de l'intelligence et au problème moral.

Il est souvent question dans ce livre du finalisme de la vie : deux chapitres sont consacrés à l'étude de *la base mnémonique du finalisme de la vie*. Est-ce à dire que l'auteur qui a institué récemment dans *Scientia* une vaste enquête sur le vitalisme et le matérialisme, soit devenu vitaliste ? Il prétend plutôt prendre une position de conciliation entre les deux courants opposés, et appelle sa théorie vitalistico-énergétique. Mais il se tient fortement parmi les finalistes. « A l'ancienne tendance anthropomorphique — ou des tendances analogues aux tendances humaines — a succédé aujourd'hui... la tendance opposée, et non moins périlleuse, d'oublier l'homme, c'est-à-dire d'oublier que l'homme avec tous ses besoins, ses activités et ses aspirations, existe, que lui et ses manifestations finalistes sont un fait non moins réel que la précipitation d'un sel ou la coagulation d'un colloïde » (p. 18).

Rignano considère justement sa théorie « comme une première tentative, à peine esquissée et tout à fait provisoire, faite surtout dans le but de rappeler sur telles questions l'attention des biologistes ». Il n'est pas impossible que les recherches qu'elle est de nature à susciter en rendent caduc précisément ce qui constitue sa parenté avec le vitalisme.

L. Guinet.

Mager, Henri. — *Une science nouvelle : la science des vibrations atomiques.* XVIII+151 p., 50 fig., Dunod, Paris, 1923. [16 Fr.]

L'histoire de la baguette divinatoire et des procédés employés par les sourciers semble enfin sortie de la période empirique. Les résultats obtenus depuis 1910, ceux surtout de l'enquête poursuivie en 1913 par l'Académie des sciences, et dont il n'est d'ailleurs point question ici,

enfin les expériences faites à Paris du 19 au 23 juin 1923, les résultats officiellement contrôlés semblent montrer qu'une nouvelle école de prospection s'est levée, qu'elle s'est affranchie radicalement de toutes les traditions empiriques du passé, et que grâce à l'emploi systématique des « détecteurs colorés » de MAGER (détecteurs de vibrations atomiques), il soit possible d'établir la carte du champ vibratoire accompagnant une eau souterraine ou un gîte minier. Les travaux de MAGER et de son école semblent devoir faire de l'antique baguette que l'on voit déjà entre les mains d'un homme dans un dessin qui remonte à 1420 (KLINCKOWSTROEM) un instrument très précis de prospection et d'analyse.

C'est cette science nouvelle, découverte par HENRI MAGER « prospecteur », dont l'exposé fait l'objet de ce livre, lequel forme suite aux ouvrages antérieurs de l'auteur : *Les sourciers et leurs procédés*, 322 p., 2e éd. 1922; *Les baguettes des sourciers et les forces de la nature*, 423 p., 1920; *Les influences des corps minéraux*, 236 p., 1914; *Hydrologie souterraine, moyens de découvrir les eaux souterraines et les utiliser*, 776 p., 1912. C'est à l'expérience seule qu'il appartient de se prononcer sur la valeur des procédés de MAGER qui, avec la belle foi naïve d'un inventeur, se compare à LE VERRIER (p. XXXVI), et de dire si effectivement ses « détecteurs de vibrations atomiques permettent : au physicien, l'étude intime de la matière, des atomes et des molécules, de l'éther et de ses vibrations; au sourcier, la recherche des gîtes miniers; à l'analyste, l'analyse de tous les corps visibles et invisibles; au biologiste, l'étude de la graine, de l'œuf et des origines de la vie ».

On pourrait évidemment chicaner l'auteur lorsqu'il étend ses procédés, qui d'ailleurs ne sont point décrits clairement dans ce livre, à l'étude de la matière et de ses constituants, affirmant que les isotopes du lithium, du bore, du néon, du magnésium, du silicium, du chlore, du brome, du krypton, du xénon, du mercure, n'existent pas, sans d'ailleurs qu'il apparaisse pourquoi trouvent grâce ceux du nickel, du zinc, du sélénium, du rubidium ou de l'étain ! On pourrait trouver aussi que c'est aller trop vite en besogne qu'affirmer « l'inanité » de la théorie de BOHR, de celle de SOMMERFELD, celle de LANGMUIR.

Mais ses recherches, celles de son école, ont donné en géologie des résultats certains. Il n'est pas impossible que leur domaine soit un jour étendu; les faits révélés par les détecteurs de MAGER méritent d'être étudiés avec le plus grand soin, mais aussi avec l'esprit critique qui souvent fait défaut à l'inventeur. Le nom de HENRI MAGER occupera peut-être un jour une place dans l'histoire des recherches sur la constitution de la matière; il ne doit pas pour le moment, me semble-t-il, sortir de l'histoire de la géologie.

L. GUINET.

Lecat, Maurice. — *Bibliographie de la Relativité.* Suivie d'un appendice sur les déterminants à plus de deux dimensions; le calcul des variations; les séries trigonométriques et l'azéotropisme. Avec la collaboration de M^me^ M. LECAT-PIERLOT. XII+292+48 p. Bruxelles, LAMERTIN. 1924.

L'examen d'ensemble de cette bibliographie très complète (trop complète peut-être) est facilité par un résumé statistique à la page 201. (Et soit dit en passant, toute bibliographie devrait être complétée par un tel résumé). J'en extrais les données suivantes. Les auteurs considérés (à l'exception des anonymes) sont au nombre de 1175. Il se répartissent comme suit par nationalités en pourcentage : Allemagne 30; Angleterre 15; France 13; États-Unis 11; Italie 5.5; Hollande et Autriche 4; Suisse 3; etc. Les travaux considérés sont au nombre de 3,775. Voici leur dénombrement par langues en pourcentage : Allemand 38; Anglais 30.5; Français 18.5; Italien 5.7; Hollandais 3.4; etc. Donc quelqu'un ne connaissant que les deux premières langues aurait accès à 68.5 p. c. de toutes ces publications; s'il connaissait aussi le français il pourrait en lire 87 p. c., et s'il connaissait en plus de cela l'italien, 92.7 p. c. Les 7.3 p. c. restant sont représentés par non moins de *dix-sept* langues. Cet excellent exemple (excellent parce que la relativité est devenue une partie essentielle de la pensée moderne et parce que les nombres considérés sont suffisamment grands) prouve que le problème des langues n'est pas aussi difficile à résoudre qu'il le paraît à première vue : il faudrait connaître, il est vrai, vingt et une langues pour lire *tous* les écrits sur la relativité, mais il suffirait d'en connaître deux pour en lire près des trois quarts et d'en connaître quatre pour en lire 92.7 p. c. La supériorité des grandes langues serait encore plus évidente s'il était possible de tenir compte de la qualité des travaux et de leur attribuer des poids. Car les grandes langues attirent nécessairement les travaux les plus essentiels : un Tchèque voulant expliquer au monde entier une théorie nouvelle sera très fortement tenté de le faire dans une des grandes langues plutôt que dans la sienne, et il choisira de préférence l'une des deux ou des trois premières langues. Il est très probable que les travaux écrits en Catalan, en Chinois ou en Ukrainien sont des travaux de vulgarisation dont l'intérêt est purement local. Toutefois, si un travail vraiment original était publié dans l'une des petites langues, il est très probable qu'il serait rapidement traduit dans l'une des grandes langues. En résumé, celui qui sait lire les trois ou quatre langues principales a accès à la totalité des travaux scientifiques, ou peu s'en faut. L'activité extraordinaire de M. LECAT nous permet de formuler une autre conclusion. Le fait que la théorie de la relativité qui n'aura pas encore vingt ans au moment où ces lignes paraîtront, a déjà causé la publica-

tion de près de 4.000 travaux est tout simplement effrayant. Mais que le lecteur se rassure, il lui suffira d'en lire au plus une douzaine bien choisis pour savoir l'essentiel. Un travail comme celui de M. LECAT devrait donc être complété par une courte bibliographie critique choisie. Mais il vaut mieux le remercier de ce que nous lui devons déjà, de la peine immense qu'il s'est donnée. Si son étude bibliographique n'avait d'autre intérêt que les conclusions statistiques résumées plus haut, elle serait déjà justifiée. Un autre travail de M. LECAT, qui présente le même intérêt statistique a été analysé dans *Isis*, IV, 444.

G. S.

Garcia del Real, Eduardo. — *Historia de la medicina en España*. 1148 p., in-8°, Editorial REUS, Madrid, 1921. [25 pes.]

L'auteur de cette *Histoire de la médecine en Espagne*, professeur d'histoire de la médecine à l'Université de Madrid, a réuni dans ce volumineux travail à peu près tout ce que l'on sait sur le sujet qui l'intéresse, mais sans s'embarasser d'une indigeste et vaine érudition; — je souhaiterais même que les indications bibliographiques fussent plus nombreuses.

Le volume s'ouvre par des considérations sur les populations primitives de l'Espagne, leurs mœurs, leurs caractères; GARCIA DEL REAL relève après RODRIGO MÉNDA DA SILVA, la coutume d'indiquer sur la porte des habitations la nature des remèdes qui avaient sauvé les occupants, et l'influence qu'eut vraisemblablement ce fait sur les Grecs colonisateurs lorsqu'ils dédièrent à Diane le temple d'Ephèse, à Esculape celui d'Epidaure. Il pense aussi, avec ALIBERT, que la médecine basée sur une philosophie purement naturaliste a puisé indirectement ses origines en Espagne. C'est aux Espagnols que l'on doit la découverte des propriétés médicinales de quelques plantes, telles que la bétoine (qui tire son nom de la Bétique), la centaurée, celle de la composition de remèdes fameux en leur temps, comme la drogue aux cent herbes.

Mais les Phéniciens, les Grecs, les Carthaginois, les Romains, ne tardèrent pas à exercer à leur tour sur l'évolution de la médecine en Espagne une influence profonde, d'ailleurs mal connue : école des Asclépiades en Andalousie, coutume d'exposer les malades sur la place publique, etc.

Après cette période héroïque, l'auteur peut enfin citer quelques noms : ANTONIUS MUSA, COLUMELLE, SÉNÈQUE LE PHILOSOPHE sont nés en Espagne. Les temples consacrés aux dieux de la médecine se multiplient, et les vestiges encore existants de thermes construits par les Romains prouvent que ces établissements étaient nombreux. Alors que l'exercice de la médecine n'est pas permis aux femmes à Rome, quel-

ques unes exercent la médecine en Espagne et y méritent le titre de « sapientissimas ».

Mais avec la destruction de l'empire romain et la conquête des Wisigoths, la culture décroit rapidement en Espagne, les écoles se ferment, et l'on ne sait plus rien de l'exercice de la médecine jusqu'au IX^e^ siècle, au moment où apparait l'influence arabe, et, par elle, de nouveau l'influence grecque. Et ce sont alors les grandes personnalités de la science judeo-arabe : AVICENNE, AVENZOAR, AVERROES, MAÏMONIDES, etc., en même temps que se fondent des hôpitaux et que s'ouvrent les universités (la première université espagnole est celle de Palencia, ouverte en 1212 ou 1214 par ALPHONSE VIII).

Et voici enfin venir les grandes figures de la médecine et de la philosophie : ARNALDO DE VILLANOVA, RAIMONDO LULIO, RAIMUNDO DE SABUNDE, qui précèdent le XV^e^ siècle, lequel est pour la médecine espagnole une période d'éclipse que n'arrivent pas à éclairer les noms de ALFONSO CHIRINO, FERNAN GÓMEZ, PEDRO PINTOR, GASPAR TORELLA, LÓPEZ DE VILLALOBOS. C'est néanmoins pendant cette période que s'organise l'enseignement qui va préparer ce que l'auteur appelle l'âge d'or de la médecine espagnole (1500-1666), qui est en même temps l'âge d'or de la culture générale, de l'art et des sciences, et auquel GARCIA DEL REAL consacre cinq chapitres que se partagent les anatomistes précurseurs de HARVEY, les chirurgiens, les médecins à proprement parler, et les médecins commentateurs et philosophes.

Parmi les anatomistes se détachent MONTÀNA DE MONSERRATE, JUAN VALVERDE, PEDRO GIMENO (élève de VESALE et de SYLVIUS), LUIS COLLADO, ANDRÈS LAGUNA, MIGUEL SERVET.

A cette brillante période succède une ère de décadence de la culture en Espagne, décadence d'ailleurs singulièrement exagérée, dit l'auteur, par les historiens espagnols eux-mêmes. Et ici, GARCIA DEL REAL s'échappe du sujet de son livre et parle des botanistes, des zoologues, des chimistes, des littérateurs, des musiciens, pour en revenir aux chirurgiens, aux médecins hydrologues, aux médecins militaires, et enfin à la réforme de l'enseignement médical qui prépare le XIX^e^ siècle auquel la moitié de l'ouvrage est consacré, ce qui détruit, à mon sens, l'équilibre de l'ouvrage.

Quoiqu'il en soit, la documentation de l'auteur est abondante; son œuvre est vivante parce qu'elle est sans cesse mêlée à la vie, et alors que la poussière des noms cités pourrait faire craindre une certaine monotonie, le livre se lit très facilement. Malheureusement l'absence d'un index en rend la consultation extrêmement difficile.

L. GUINET

Bergson, Henri — *Durée et simultanéité, à propos de la théorie d'*EINSTEIN. Deuxième édition, augmentée. x+289 p. Paris, ALCAN, 1923.

The concept of time has been subjected recently to two revolutionary analyses — one philosophic, in the Intuitional epistemology of BERGSON; the other scientific, in the Relativity Theory of EINSTEIN (not to mention the Quantum Theory, with its possible demand for a «discontinuous» time). The volume under review constitutes a searching examination on the part of BERGSON to discover whether, and if so how far, his own conception of duration harmonizes with EINSTEIN's ideas on time. Is pure duration — which is, according to BERGSON, a direct and immediate experience, and agrees with the natural and ordinary belief in a unique and universal time — is duration, asks our author, compatible with the Relativity notion of time, time which seems to bristle with paradoxes, time which, as we change our point of view, flows now fast now slowly, is now a simultaneity now a succession? What has philosophy to say to such a concept? « Aucune question n'a été plus négligée par les philosophes que celle du temps; et pourtant tous s'accordent à la déclarer capitale. » It is thus the time-concept, and that alone, in the Theory of Relativity to the analysis of which BERGSON limits himself in this volume.

In the first two chapters the author considers the special Theory of Relativity under two aspects, which he calls unilateral and bilateral. In the study of the well-known LORENTZ formulas and their logical consequences, and the difficult concepts that are now the commonplaces of special Relativity, BERGSON has one prime object in view — to discover the *concrete meanings* of the terms which enter into the various formulas. Again, in chapters three and four, in his examination of the nature of time and of the plurality of times, and of the 'paradoxes' usually associated with Relativity-time, what concerns him as a philosopher is not the mathematical « transformations » but the meaning beneath the symbolic procedure of the agile mathematician. The same spirit of inquiry into concrete reality underlies his discussion, in the last two chapters, of the relation between light and space, and of four-dimensional « space-time ».

Are, then, the various Relativity-times compatible with a belief in a unique and universal time? Yes, answers BERGSON, *provided* we are careful to make the distinction that constitutes the very essence of his philosophy — provided we distinguish between time as *experienced* and time as *measured*. Multiple time as it enters in the calculations of the physicist is, really, not time itself but its measure — ultimately, it is only a *spatialized* measure of real time. Once we recognize the superficial character of the analogy between space and time, between

quantity and quality, we cannot escape the conclusion that time requires a mode of treatment altogether different from that of space. The distinction between experienced time and measured time removes from Relativity all appearance of paradox.

Although BERGSON's examination of Relativity is limited to the special theory, his results, so far as they bear on the concept of *time*, hold equally, he maintains, in EINSTEIN's general theory (and, by implication, in any possible Relativity theory based on the notion of a mathematical space-time).

The text of the present edition is the same as in the first, except for the addition of three appendices, which deal with certain technicalities. These appendices are intended to show that, as concerns *time*, there is no difference between a system in uniform rectilinear motion and a system moving in any way whatsoever.

This book may be recommended to scientists as an introduction to Bergsonian philosophy.

(*Cambridge, Mass.*) HENRY M. SHEFFER.

Claparède, Edouard. — *Comment agnostiquer les aptitudes chez les écoliers.* 300 p., in-16. Bibl. de philos. scientifique. ERNEST FLAMMARION, Paris, 1924. [8 Fr.]

La question que le savant psychologue genevois examine dans ce livre, où il reprend d'ailleurs certains de ses exposés antérieurs (1), est d'importance primordiale non seulement pour l'enfant, mais pour l'organisme social tout entier, étant donné que dans les circonstances économiques actuelles, plus que jamais chaque nation a intérêt à tirer le meilleur parti possible des citoyens qui la composent, à ne pas gaspiller les intelligences supérieures en ne leur fournissant pas la culture qui leur conviendrait, à ne gaspiller ni temps ni argent en donnant une culture supérieure à ceux qui ne peuvent en tirer profit, à tirer des anormaux aussi bien que des normaux le meilleur parti possible.

Est-ce à dire qu'une réponse absolument formelle puisse dès maintenant être apportée à cette question, qui est en réalité non seulement une question de diagnostic, mais aussi une question de pronostic? Que non pas. Depuis les premiers essais faits en 1885 par RIEGER, de Würzburg, pour déterminer par l'expérimentation les aptitudes mentales d'un individu, les recherches de LOMBROSO, OERHN, CATTELL, MUNSTERBERG, BOLTON, JASTROW, SCRIPTURE, BINET et HENRI, TOULOUSE,

(1) Voir en particulier : *Psychologie de l'enfant et pédagogie expérimentale*, 10e éd., 1924, et les *Archives de Psychologie*, 1901 et suiv.

Guicciardi et Ferrari, Sommer, Gilbert, Bourdon, Ebbinghaus, qui s'étendent sur une période de vingt ans, n'avaient donné que des directives à peu près inutilisables, les épreuves proposées n'étant jamais accompagnées d'échelles de graduation. En 1905 paraît enfin l' « Échelle métrique de l'intelligence » de Binet et Simon (1), qui, à part quelques railleries, passe tout à fait inaperçue en France, mais soulève l'enthousiasme à l'étranger, et devient la source de très nombreux travaux qui ont enfin permis de mettre la main sur des procédés permettant de répondre, au moins partiellement, à la question qui fait l'objet de ce livre.

Claparède a donc groupé, pour les éducateurs, les médecins, et tous ceux qui s'occupent d'éducation professionnelle et pédagogique, les meilleurs parmi les moyens de diagnostic proposés, et expose la méthode propre à faciliter les nouvelles recherches qui s'imposent dans ce domaine.

Après trois chapitres de généralités sur les aptitudes et leur évaluation (107 p.), il passe en revue toute une série de tests de développement et d'aptitudes empruntés à différents auteurs : Binet et Simon, Decroly, Terman, de Sanctis, Descoeudres, Bovet, Porteus, Rorschach, Whitley, David, Yerkes, etc., qu'il a eu l'occasion de mettre en œuvre ou de faire mettre en œuvre, notamment à l'Institut J.-J. Rousseau (de Genève) de modifier parfois, et dont il montre aussi bien les points faibles que les qualités.

L'ouvrage ainsi constitué représente, non pas un outil que peut prendre en main le praticien, mais une introduction à la façon de se créer cet outil. Encore faut-il noter que dans ce domaine la mise en œuvre de tests peut être extrêmement dangereuse, surtout lorsqu'il s'agit d'établir un pronostic pour ceux qu'on appelle « les mieux doués », si le praticien n'est pas un psychologue averti et n'est pas doué d'un tact infiniment délicat. Peut-être cette difficulté n'est-elle pas ici suffisamment mise en lumière

L. Guinet.

Dingler, Hugo. — *Die Grundlagen der Physik. Synthetische Prinzipien der mathematischen Naturphilosophie.* xiv+336 p., Berlin und Leipzig, Walter de Gruyter, 1923.

Is it possible to construct a philosophy of physics that shall avoid both the Scylla of inapplicable Rationalism and the Charybdis of untrustworthy Empiricism? Professor Dingler believes it is. In

(1) Voir surtout Alfred Binet et Th. Simon : *La mesure du développement de l'intelligence chez les jeunes enfants*, 78 p., 25 pl., publié par la Société Alfred Binet, 36, rue de la Grange-aux-Belles, Paris.

the volume under review, which is a second, completely revised edition of a book published originally in 1919, the author presents a detailed exposition of a theory which he calls 'the principle of pure synthesis'. The logical basis of this theory, and the central theme of the book, is 'the principle of exhaustion'. This seems to the reviewer to be a highly ingenious attempt at combining a coherence theory of truth with a nominalistic theory of concepts. In terms of the principle of exhaustion Professor DINGLER develops the theory of synthetic logic, mathematics, mechanics, and physics. He lays special stress on the practical workability of his synthetic philosophy. We may mention in particular two of his important applications: MENDEL's laws, and the theory of probability.

One of the most far-reaching consequences of the author's doctrine is the principle which he formulates as 'the well-ordering of empirical concepts'. To avoid circular definitions, we must order our empirical, and above all our experimental, concepts in a definitional hierarchy. Failure to observe this caution results, so DINGLER maintains, in circular definitions of the nature of space, matter, etc., in the Theory of Relativity.

True to his synthetic ideal, DINGLER is careful not to be misled by what may be called mathematical aesthetics. That a *mathematical* demonstration can be carried through validly, or even 'elegantly', is never for him a proof of a corresponding *physical* fact; for, invalid as well as valid physical procedures may be represented by valid mathematical formulas. To the scientist, mathematics is always form, and never content; to the physicist, mathematics is only *Forschungsinstrument*.

From the standpoint of current mathematical logic and philosophy of science there is much in the present volume deserving of commendation. Of outstanding logical value is the constant emphasis upon the multiplicity of possible empirical interpretations of any single formal structure.

(*Cambridge, Mass.*). HENRY M. SHEFFER.

Elsbach, Alfred C. — KANT *und* EINSTEIN. *Untersuchungen über das Verhältnis der modernen Erkenntnistheorie zur Relativitätstheorie.* VIII+374 p. Berlin und Leipzig, WALTER DE GRUYTER, 1924.

Relativism in speculative philosophy and Covariance in theoretical physics — what have these concepts in common? Not very much. Replace, however, the term 'relativism' by 'philosophical relativity',

and the term 'covariance' by 'physical relativity', and then carelessly drop the adjectives, and there results, not one concept but one word : Relativity.

How does the Theory of Relativity, meaning the Theory of mathematico-physical Covariance, bear on contemporary theories of knowledge? In particular, how does the theory, especially in its views on space and time, bear on the neo-Kantian epistemology, as developed by HERMANN COHEN, PAUL NATORP, and ERNST CASSIRER? This is the question to which the detailed and searching discussions in the present volume are intended to furnish an answer.

In the first part of the book ELSBACH expounds the 'critical' theory of knowledge, basing his discussion essentially on CASSIRER's *Substanzbegriff und Funktionsbegriff (Isis* VI, 439). He devotes his chapters to such problems as the object of knowledge, the concept of truth, the formation of concepts, the structure of theoretical physics, and the task of theoretical philosophy. In the second part, the author examines the relation of the Theory of Relativity to this critical theory of knowledge. For this purpose he presents, first, an analysis of the views of COHEN and of NATORP on space and time; secondly, a thorough exposition of the philosophy of CASSIRER. As might be expected, he considers such problems as absolute space, absolute time, the relation between space and time, matter, and non-euclidean geometry. The book closes with three pages of bibliography.

What, then, is the relation between KANT and EINSTEIN? According to the author, there is *necessarily* a complete agreement between the philosophy of the neo-Kantian school and the latest teaching of mathematico-physical science. Those who are tempted to think otherwise are misled by the fact that the champions of the two theories speak not only difficult but fundamentally different languages. It is emphasis upon this notion of structural unity underlying analytico-linguistic diversity that seems to the reviewer to constitute one of the great advances in contemporary philosophy of science, and one of the merits of the present book.

(*Cambridge, Mass.*) HENRY M. SHEFFER.

Ferrière, Adolphe. — *La loi du progrès en biologie et en sociologie et la question de l'organisme social.* Etude précédée d'une introduction philosophique sur la méthode en sociologie. XII+680 p., in-8°. GIARD ET BRIÈRE, Paris, 1915. [30 Fr.]

Bien que cet ouvrage du directeur du Bureau international des Écoles Nouvelles (Genève) soit déjà quelque peu ancien, je crois inté-

ressant de le signaler ici parce qu'il me semble que FERRIÈRE y pose les bases méthodiques de la biologie sociale.

Le volume s'ouvre par une longue étude (80 p.) des principes de la méthode en sociologie : pour l'auteur, la sociologie rentre dans le domaine des sciences de l'esprit; mais elle se place sur le terrain de la science pratique; elle ne dédaigne certes pas les ressources de la méthode expérimentale, mais la méthode de la science pragmatique seule peut lui permettre de constituer une science indépendante.

FERRIÈRE pose ensuite la question de l'assimilation de la société à un organisme, et c'est l'occasion d'un exposé historique dans lequel il examine les thèses de COMTE, SPENCER, LILIENFELD, ESPINAS, DE GREEF, WORMS, FOUILLÉE, WAXWEILER : sa conclusion est que tout parallélisme *statique* entre les organes des êtres vivants et ceux des sociétés est un symbolisme vide de sens; seules sont réelles des analogies d'ordre *dynamique* qui proviennent de l'origine commune des phénomènes sociaux et des phénomènes organiques en tant que phénomènes de vie.

Ceci étant, il se livre à l'étude de l'individu d'une part (pp. 195-328), de la société d'autre part (pp. 329-546) au triple point de vue de l'être : « dans la société, comme dans les organismes, l'unité est caractérisée par la convergence interne des activités, les éléments distincts collaborant en vue de maintenir l'intégrité du tout dont ils font partie »; du devenir : « dans les sociétés comme dans les organismes, le devenir est caractérisé par la tendance des unités à faire converger leurs activités internes vers la conservation et l'accroissement de leurs énergies, fin suprême de l'élan vital qui les anime », et du progrès : « les sociétés comme les organismes se conforment d'une part à l'adaptation de soi au monde et du monde à soi, d'autre part à la tendance qui les pousse à conserver et à accroître leur puissance vitale », le progrès vital étant envisagé d'abord en général, puis comme progrès politique, comme progrès juridique et comme progrès économique.

La place m'est trop mesurée pour que je puisse entrer dans le détail de l'ouvrage, qui embrasse en somme presque tout l'ensemble des sciences biologiques, psychologiques et sociales, l'étude de ces dernières étant de beaucoup la plus développée. Je note cependant qu'à propos du devenir social, il examine les trois étapes du progrès dans les sociétés : régime de l'autorité acceptée, de l'anarchie relative, de la liberté réfléchie; que l'esprit des masses et le rôle des élites sont l'occasion de très fortes études psychologiques sur l'homme médiocre et les partis politiques, sur le meneur, sur l'homme d'élite, etc.

Partout, FERRIÈRE voit s'exalter « le rôle actif de l'humanité dans l'effort : effort intellectuel tendant à la clairvoyance, à la raison et à la justice, effort de la volonté tendant au triomphe de l'esprit sur la

matière » (p. 656). Or il n'y a pas d'effort efficace sans connaissances psychologiques, et pas d'accroissement de puissance de l'esprit sans effort dirigé selon les lumières d'une raison supérieure, sans morale. Ainsi donc, l'intérêt bien entendu se confond, au terme, avec la morale la plus haute et condamne la politique réaliste, l' « égoïsme sacré » des nations.

Tout au long du livre, je retrouve, associés à une grande rigueur scientifique, le même idéalisme, le même ton de foi et de bonne foi.

L. GUINET.

Clara Endicott Sears. — *Days of delusion.* A strange bit of history. XXVIII+264 p., many illustrations. Boston, HOUGHTON MIFFLIN, 1924.

Some years ago, I had planned the writing of a series of studies on the history of human folly and I had already accumulated considerable material relative to the first one to be entitled *The End of the World*. I thought it would be interesting to make a comparative survey of millenarian and similar fancies, such as have occurred periodically throughout the course of recorded history, and not simply among Jews and Christians but among people of every faith. I soon discovered that the subject was considerably vaster than I had at first realized, but I did not loose courage and I would probably have accomplished my purpose had it not been suddenly thwarted by the loss of all my notes while travelling in Europe. The prophecies of WILLIAM MILLER, a Massachusetts farmer, were of course a part of my investigations and I had found in the Harvard Library not a few documents dealing with them. Yet what I knew about them did not amount to very much. The appearance of Miss SEARS' book, which is entirely devoted to the study of the Millerite delusion, thus gave me considerable pleasure. At the same time, it made me realize once more that the loss of my notes was of no particular importance.

Miss SEARS has had the excellent idea of appealing to witnesses of the great religious excitement of 1843, or to their children, in order to collect and edit their reminiscences. She did it at the right time-early enough to preserve genuine recollections, late enough to avoid the reawakening of rancour and prejudice. For the benefit of our European readers, it may be well to recall briefly that WILLIAM MILLER (born in 1782 in Pittsfield, Western Massachusetts) prophesied that CHRIST would reappear a second time about the vernal equinox of 1843, when the day of judgment and retribution would occur. He was a farmer who, after a few years of intellectual dissipation, took part in the war with England in 1813-14, returned home converted,

and spent many years studying the Bible and making extraordinary computations. The upshot of all his Biblico-mathematical research was that the world would end on or about March 20, 1843. The fear of an impending judgment was intensified by various meteorological wonders : an unparalleled display of falling stars sometime before dawn on Nov. 13, 1833; extraordinary solar haloes and parhelia; a great comet in 1843. MILLER was a very sound man in every respect except one; he did not hurry to spread his theories but remained on his farm, working his land, bringing up ten children, studying and pondering all the time. He did not begin his preaching until he was fifty and even then, this beginning was accidental : he had been asked to replace a minister in an emergency. For many years his preaching was restricted to the country-side until, in 1840, he chanced to meet the Rev. JOSHUA V. HIMES, who took him to the cities and became, so-to-say, his manager. MILLER's deep study of the Bible gave him a tremendous advantage upon the New England ministers, whose literalism was equal to his but whose Biblical knowledge was vastly inferior. Few could argue with him and controvert successfully his arguments. It is possible, however, that among those who invited him to address their congregations, many did not really believe in his prophecies. They were impressed by MILLER's obvious goodness and fervour, by his irresistible knowledge of the Holy Book. Many, who were building new churches and found it hard to shake the sluggishness of their congregations, thought perhaps that it would do them no harm to become more directly aware of an impending day of reckoning. When one is not very strictly honest in such matters — (much more so than the theological education of those days allowed) it is well-nigh impossible to say where honesty ends and dishonesty begins : there is between them a sort of no-conscience's land, where such distinctions gradually fade. MILLER was profoundly honest, but it is probable that many of the ministers who supported him were not equally honest, perhaps, without being aware of any dishonesty. A preacher like MILLER was a godsend; the « sinners' benches » and the « anxious seats » were crowded when he spoke: the congregations woke up; their religion was exalted : was this not a pure gain ? The few ministers who had at once enough common sense and courage to stem the tide of folly deserve our full admiration — men like the Bishop of Vermont, the Rev JOHN HENRY HOPKINS, D. D. and the Rev. ABEL C. THOMAS of Lowell — but there were too few of them and they were overwhelmed by the deluded crowds. When the shepherds loose their heads, can we blame the flocks ?

« It has been estimated », says Miss SEARS, « that the number of sincere and genuine Millerites numbered 50,000, but added to that

were hosts of followers who tentatively believed, and followed the real believers in a state of terrified uncertainty. They were as loud in their expressions of conviction as any, but when the end failed to come they dropped away, denying ever having taken part in the movement. There were also great numbers who became followers mainly from a love of excitement, who attended all the meetings taking place in their neighborhood, and up to the end did their full share of spreading fanaticism right and left. The imminent approach of Doom did not concern these so much as the morbid and, to them, pleasurable excitement of preparing for it. » The recital of the many events which occurred on the last eve is a curious mixture of comedy and tragedy. We see people marching in white robes to elevated places or spending the night on their roofs or in treetops to be more ready to rise. It is interesting to note that many women were more sensible than the men; they did not surrender as completely to the common delusion. They believed that the last eve had come and put on their white ascension robes, yet they baked bread for the morrow maugre their husbands' injunctions!

I have said enough to show that Miss SEARS' excellent book will prove of great interest to psychologists and to students of human thought. Historians of science will be reminded by it of the very frailness of our intellectual culture; it is too new to be deepseated (1). The events of the war have shown that the mental equilibrium even of great men of science is not very stable and that their critical faculty can be suddenly paralyzed. The consciousness of this will help the historian to be tolerant, patient and kind.

GEORGE SARTON.

Wheeler, William Morton. — *Social life among the insects*. 375 p., 116 fig. HARCOURT, BRACE and Co. New York, 1923.

Forel, Auguste. — *Le monde social des fourmis du globe comparé à celui de l'homme*. 5 tomes. Tome I, 192 p, 3 pl., 30 fig, 1921. Tome II, 184 p., 4 pl., 38 fig., 1921. Tome III, 227 p, 10 pl., 28 fig., 1922. Tome IV, 172 p., 4 pl., 11 fig, 1923. Tome V, 174 p. 3 pl., 30 fig., 1923. KUNDIG. Genève.

Le livre du Professeur WHEELER est un excellent résumé des connaissances actuelles sur les vingt-quatre différents groupes d'insectes

(1) See my reviews of BUTLER's Solar biology, *Isis* III, 449; of LAWRENCE's Spiritualism, *Isis* IV, 567, etc. and in every Critical Bibliography the chapter on Superstition and Occultism.

sociaux ou subsociaux. Le thème de ce beau travail est l'explication des mœurs sociales par l'évolutionnisme. Ainsi les instincts des Sphex, que J. H. Fabre avait employés comme argument contre l'évolution, sont ramenés à des réflexes purement mécaniques, ayant apparu comme effets des lois générales de la phylogénie. L'auteur trace de même le développement des collectivités des guêpes et des abeilles, depuis les simples insectes solitaires jusqu'aux colonies les plus complexes.

Un des faits qui est le plus employé comme explication des réactions de beaucoup d'insectes sociaux, notamment les fourmis et les termites, est la trophollaxie. Elle consiste en un échange de certaines substances trophiques, non seulement entre adultes et larves, mais aussi entre différents adultes, et entre ceux-ci et leurs hôtes myrmécophiles ou termitophiles. Ainsi l'altruisme social, du moins parmi les insectes, ne serait que la recherche de certaines sensations agréables!

Les six chapitres représentent une série de conférences, faites à Boston, au « Lowell Institute », en 1922. Après quelques remarques générales sur les sociétés des insectes, l'auteur décrit les colonies peu connues de Coléoptères, puis donne un aperçu des guêpes et des abeilles solitaires et sociales. Deux chapitres sont consacrés aux fourmis, un sujet dont l'auteur parle en maître, puisque c'est à lui qu'on doit beaucoup des belles découvertes qu'il décrit. Un chapitre sur les termites, puis une bibliographie très complète, terminent le livre.

Le travail du Professeur Forel se borne à ceux des insectes que Wheeler a appelés « the dominant social insects » — les fourmis. Quoique beaucoup plus détaillé que l'œuvre de l'auteur américain, ce travail est un peu plus un livre de popularisation, et ne pourrait être de grande valeur pour un chercheur sérieux. Ainsi on ne saurait y trouver de liste bibliographique. Néanmoins, à part cette lacune, l'ouvrage est assez complet. Je ne pourrais mieux donner une idée de son contenu qu'en citant la liste des titres : Genèse, formes, anatomie, classification, géographie, fossiles. Sensations, physiologie, fourmis et plantes, hôtes, parasites, nids. Appareils d'observation, fondation des fourmilières, mœurs à l'intérieur des nids, bétail, jardins, fourmis parasites. Alliances et guerres, parabiose, lestobiose, esclavagisme. Mœurs spécialisées, épilogue.

Le lecteur a donc un aperçu général du monde des fourmis, où l'auteur fait partout une comparaison entre celles-ci et les hommes. L'enseignement que Forel tire de ses observations est un appel à une parabiose terrestre, ou fédération internationale des peuples, pour

mettre arrêt aux guerres politiques et sociales. Mais il faut bien noter que normalement chez les fourmis la parabiose n'implique que peu de colonies et que la vie sociale est le prix de l'asexualisation presque totale de la colonie. Faut il croire que parmi les hommes une fédération mondiale ne sera possible que lorsqu'elle sera synchronique avec un sévère contrôle de la natalité?

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